Agricultural Research Institute

PUSA

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From February 1 to June 21, 1883.

VOL. XXXV.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.
MDCCCLXXXIII.

LONDON:
HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY,
ST. MARTIN'S LANE.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

February 1, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Electrical Resistance of Carbon Contacts." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Professor W. G. ADAMS, F.R.S. Received January 18, 1883.

1. *Object of the Investigation.*

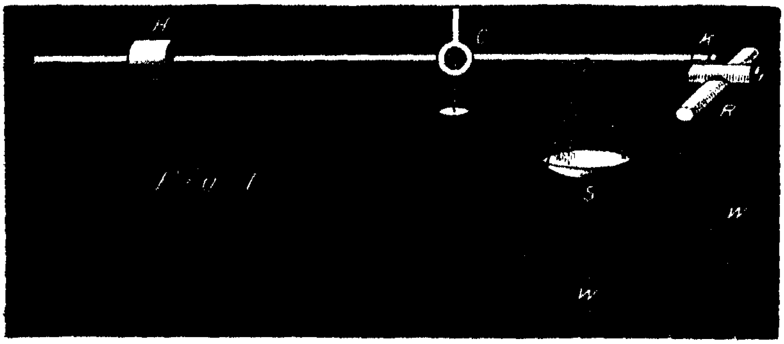
It is well known that the electrical resistance of carbon contacts diminishes with increased pressure, though, so far as I am aware, the phenomenon has never hitherto been systematically investigated. The experiments described in this paper were begun with the object of establishing a quantitative relation between pressure and resistance; but the subject grew considerably under treatment; fresh facts from time to time claimed attention, and several interesting details, not, I believe, previously observed, were eventually brought to light.

Loose contacts are proverbial for the uncertainty of their action, and it was to be expected that the investigation would prove troublesome and the results obtained by no means uniform. By multiplying experiments, however, the element of uncertainty was to a great extent removed, and in most cases it was found possible to indicate a general law with tolerable certainty.

2. *Apparatûs for Regulating Pressure.*

The instrument used for regulating the pressure is shown in fig. 1.

The apparatus consists of a little balance. HK is a light steel rod formed of a fine knitting needle balanced at C on a knife-edge, which



is fixed at right angles to it. To one end, K, is attached a split tube of thin copper for holding short rods of the different materials to be tested, which are held so as to press at right angles upon another rod, R, supported in a spring clamp. Midway between C and K is hung the scale-pan S, and at H is a sliding counterpoise, which, at the beginning of an experiment, is so adjusted that a weight of ·01 grm. in the scale-pan will just bring K and R into contact with each other. Electrical connexion is made through the wires W_1 and W_2, the former of which is attached to the clamp holding R, and the latter communicates with a mercury cup, into which dips a wire attached to the rod HK at the point C. It is evident that the pressure between K and R will be equal to half the weight in the scale-pan S.

3. *Relation between the Pressure of Carbon Contacts and their Resistance.*

Two short rods of electric light carbon, 6 millims. in diameter, were placed in the instrument in the positions shown at K and R, and the resistance with different pressures measured in the ordinary manner with a Wheatstone's bridge. The means of three series of such measurements made under as nearly as possible the same conditions, are given in Table I.

Table I.—Relation between Pressure and Resistance when measured in the ordinary way.

Pressure. Grammes.		Resistance. Ohms.
·25	....	16·10
·5	....	11·00
1	....	8·43
1·5	....	6·70
2	....	6·15

Pressure. Grammes.		Resistance. Ohms.
2 ·5	....	5 ·90
3	....	5 ·13
3 ·5	....	5 ·00
4	....	4 ·60
4 ·5	....	4 ·33
5	....	4 ·23
7 ·5	....	3 ·53
10	....	3 ·06
12 ·5	....	2 ·80
15	....	2 ·60
17 ·5	....	2 ·46
20	....	2 ·33
25	....	2 ·16
50	....	1 ·86

From the above results the curve fig. 2 has been constructed, where the abscissæ represent the pressures at the points of contact and the ordinates the resistance in ohms. The curve shows clearly that the greatest variations occur when the pressure is small and the resistance comparatively high.

4. *Change of Resistance partly due to Effect of Current.*

In making measurements of the kind above described, the details of the arrangements were from time to time varied. Different proportional coils were used and different numbers of battery cells; and it soon became evident that the resistance of the carbon contact was largely dependent on the strength of the current used to measure it. In Table II (giving the mean of two series of observations) is shown the result of using one, two, three, and four cells successively.

Table II.—Effect upon Resistance of Increased Electromotive Force.

Pressure. Grms.	Resistance in ohms with			
	1 cell.	2 cells.	cells.	4 cells.
·25	11·10	7·20	4·70	3·55
·5	5·95	4·70	4·10	3·50
1	4·40	3·65	3·25	3·10
1·5	3·60	3·20	2·95	2·80
2	3·55	3·15	2·80	2·50
2·5	3·35	2·95	2·65	2·40
3	2·90	2·55	2·35	2·30
3·5	2·45	2·30	2·05	1·95
4	2·25	2·10	2·00	1·90
4·5	2·10	1·95	1·85	1·75
5	1·95	1·85	1·75	1·70
7·5	1·55	1·55	1·50	1·45
10	1·50	1·45	1·40	1·35
25	1·15	1·05	1·05	1·05

It became interesting to ascertain what would be the effect of making still further variations in the strength of the measuring current. A box of resistance coils was therefore inserted between the battery (two Leclanché cells) and the bridge arrangement; and the weight in the scale-pan remaining the same, various resistances were successively unplugged in the box. Table III gives a few of the measurements with pressures of ·5, 2·5, 7·5, and 25 grms. This table shows that the resistance of the carbon contacts varies greatly with the strength of the current when the pressure is small, and but very slightly when the pressure is great.

Table III.

Added resistance in circuit. Ohms.	Resistance in ohms with pressure of			
	·5 grm.	2·5 grms.	7·5 grms.	25 grms.
0	5·6	2·7	1·7	1·02
5	—	2·8	1·8	1·03
10	6·9	2·9	1·8	1·03
50	7·7	3·1	1·9	1·03
100	8·6	3·1	1·9	1·03
500	9·3	3·1	1·9	1·03
1000	9·7	3·1	1·9	1·03

It is clear therefore that the diminution of resistance which occurs when the pressure between carbon contacts is increased is not, under ordinary circumstances, due solely to the direct effect of pressure. For the diminished resistance which results from the increased pressure causes an increase in the strength of the current, the effect of which is a still further fall in the resistance. Thus the total diminution of resistance is due partly to increase of pressure, and partly to increased strength of current.

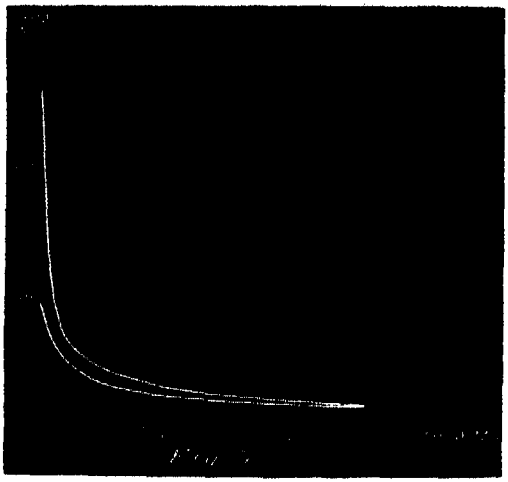
5. *Variation of Resistance with Varying Pressure and Constant Current.*

Table IV and the curves in fig. 3 show the results when special means were taken to maintain constant currents of ·1 and ·001 ampère respectively, the pressure at the contacts increasing from ·05 to 25 grms.

Table IV.—Effect of Pressure upon Resistance of Carbon Contacts with Constant Currents.

Pressure. Grms.	Resistance in ohms with current of	
	·1 ampère.	·001 ampère.
·05	11 ·02	68 ·00
·25	9 ·27	25 ·50
·5	8 ·45	17 ·75
1	6 ·56	11 ·75
1 ·5	5 ·53	9 ·75
2	5 ·34	7 ·50
2 ·5	4 ·57	6 ·50
3	4 ·15	5 ·85
3 ·5	4 ·03	5 ·70
4	3 ·63	5 ·70
4 ·5	3 ·41	4 ·95
5	3 ·31	4 ·95
7 ·5	2 ·95	3 ·65
10	2 ·51	3 ·15
15	2 ·10	2 ·45
20	1 ·89	2 ·10
25	1 ·67	1 ·75

It will be seen that with small pressures the resistance is largely dependent upon the strength of the current, but when the pressure is considerable, the resistance with weak and strong currents is nearly the same.



6. Variation of Resistance with Varying Current and Constant Pressure.

The same effect may be exhibited in another form by varying the current while the pressure remains constant. Table V gives the variations in the resistance with currents of from ·01 to ·2 ampère, the pressure at the point of contact being throughout 1 grm.

Table V.—Changes of Resistance produced by Current, the Pressure remaining Constant at 1 grm.

Current. Ampère.	Resistance. Ohms.
·01	11·4
·02	10·8
·03	10·4
·04	9·8
·05	9·6
·06	8·9
·07	8·5
·08	8·2
·09	7·9
·1	7·6
·15	5·6
·2	4·5

The results are also shown in the curve fig. 4.

7. *The Effect of Pressure on the Resistance of Carbon is Temporary.*

In order to ascertain whether the diminution of resistance under pressure was entirely of a temporary nature, or whether it continued in any degree after the pressure had been removed (the carbons of course being undisturbed) a special arrangement was made by means of which a 5 grm. weight could be placed in or taken out of the scale-pan without causing any appreciable oscillation. The weight was attached by a thread to one end of a horizontal lever, the lever being so supported that when the other end of it was depressed the weight was raised about a centimetre above the pan; this could be done without in any material degree disturbing the carbon contacts.

A weight of 1 grm. being in the scale-pan, the resistance of the contact was measured with the Wheatstone's bridge, and one Leclanché cell; the 5 grm. weight was then lowered into the pan, and a measurement made of the diminished resistance. The 5 grm. weight was again gently raised, and the resistance with a single gramme once more measured. In every case it was found to have returned almost exactly to its original value. Table VI gives six series of measurements thus made.

Table VI.—One Leclanché. Proportional Coils 10 and 1,000 ohms.

Pressure. Grms.	Resistance.					
	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	19	20	20	19·6	19·3	19·2
3	25	15	14	13·9	13·7	13·6
·5	20	20	20	19·9	19·5	20·0

The experiment was repeated with a resistance of 300 ohms between

the battery and the bridge, that the current might be smaller. The results are contained in Table VII, and are substantially the same as before.

Table VII.—Same arrangement as in Table VI, with a Resistance of 300 ohms between the Battery and Bridge.

Pressure. Grms.	Resistance.					
	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	30·1	31·5	31·6	34·4	35·4	34·1
3	24·3	25·2	19·0	19·6	19·8	18·8
·5	32·9	33·2	34·4	36·4	35·1	36·4

It will be noticed, however, that the final resistance is almost invariably slightly higher than the original resistance; but this increased resistance gradually diminished, as may be seen by comparing the last figure of one column with the first of the next, and perhaps if time were given it would return to its original value. This is probably a thermo-electric effect.

8. *Effects of Heating Contacts.*

Since, as is well known, the resistance of a continuous conductor of carbon is diminished by heat, it would be reasonable to suppose that the diminution which the resistance of carbon contacts exhibits under the influence of increased currents is due merely to the heating effect of the current. Experiments made with the view of testing this hypothesis failed, however, to support it. Carbon contacts of various forms were heated in an air-bath, and their resistance measured at different degrees of temperature. The resistance was in every case found to vary irregularly (probably because, in consequence of the expansion due to heat, the relative positions of the points of contact were slightly altered) but, in general, rise of temperature was accompanied by a rise in the resistance. Thus, in one case with a pressure of 1 grm., the resistance which at 16° C. was 9 ohms, became at 50° 10·7 ohms. Upon another occasion the resistance increased from 11·9 ohms at 15° to 15·6 ohms at 36°. And again with a pressure of 5 grms., the resistance increased from 4·7 ohms at 17° to 5·7 ohms at 50°. In every case the resistance fluctuated considerably, rising and falling alternately as the temperature was raised, but the general tendency seemed to be towards increased resistance. This may possibly be due to the formation of a non-conducting film by air or gases expelled from the carbon by heat.

9. *Permanent Effect of Current upon Resistance.*

Under certain circumstances the current produces a permanent and not merely temporary effect upon the resistance; that is to say, the resistance of the contacts (so long as they are undisturbed by vibration or otherwise,) is found to be lower or higher after the current has ceased, than it was at first.

Thus, for example, with a pressure of ·5 grm. at the point of contact, the resistance measured with a current of one or two milliampères was found to be 10·5 ohms. A current of ·15 ampère was then passed through the carbons for 10 seconds; and on again testing with the weak current, the resistance was found to have fallen to 5·8 ohms. With a pressure of 2·5 grms., the same current reduced the resistance from 6·6 to 4·3 ohms. With 5 grms., the resistance was permanently reduced from 4·7 to 3·4 ohms; and with 25 grms. from 2·4 to 2·0 ohms. All these effects were produced by a current of ·15 ampère. Stronger currents (up to a certain point) effect greater reductions.

Table VIII.

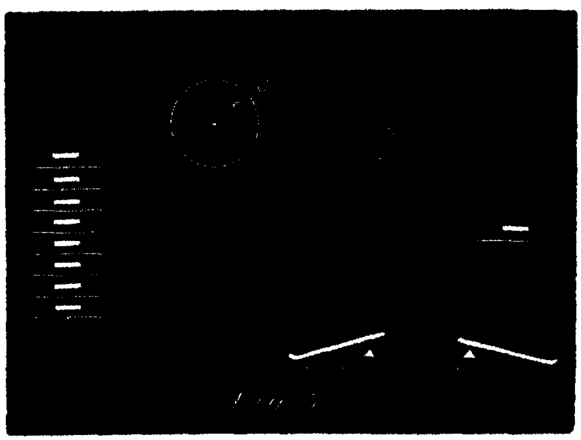
Pressure. Grms.	Smallest current with which rise occurred. Ampère.
·05	·02
·1	·05
·15	·10
·2	·19
·3	·17
·4	·29
·5	·37
1	·40
1·5	·43
2	·47
3	·57
4	·51
5	·63

But when the proportion of the current strength to the pressure exceeds a certain limit, the effect of the current is apparently to produce a permanent *increase* in the resistance, and this increase is generally very considerable. The carbon contacts, a tangent galvanometer, a box of resistance coils, and a battery of ten Leclanché cells are connected in simple circuit, and the strength of the current gradually increased by plugging out resistance in the box. As the

current increases, the deflection of the galvanometer needle becomes greater until a certain strength is attained, when the galvanometer needle suddenly goes back, and comes to rest almost or exactly at zero. The lighter the pressure the smaller the current necessary to produce this effect. Table VIII gives the currents with which the rise occurred, the pressure at the points of contact varying from ·05 grm. to 5 grms.

Attempts were made to measure the high resistance with a Wheatstone's bridge and a small current, and this brought to light the curious fact that the resistance in question is generally very much lower for weak than for strong currents. Thus it may happen that while the moment before measurement the resistance may be sensibly infinite, the very act of measurement reduces it to a few hundred ohms.

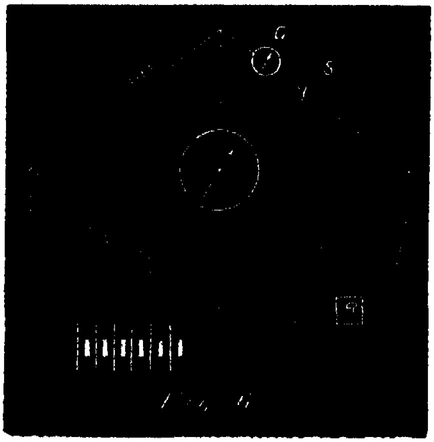
This paradoxical effect is best shown by the special arrangement indicated in fig. 5.



G is the tangent galvanometer, S the carbon balance in the scale-pan of which is a weight of 1 grm. When key 1 is depressed the current from eight Leclanché cells passes through the galvanometer and the carbons. The immediate effect is a deflection of (in the case of my galvanometer) about 75°. Almost instantly, however, the needle returns to zero, and key 1 may now be raised and depressed without producing the slightest movement of the needle. If, however, key 2 is depressed the current from a single cell will pass through the galvanometer and the carbons, and this will generally produce a small deflection of from 2° to 10°. But if key 1 is once more depressed the needle again altogether fails to move. The resistance at S is therefore of a very peculiar nature, being more

easily overcome by a small electromotive force than by a high one. Though the experiment as thus described is generally successful, it occasionally happens that the single cell fails equally with the larger battery to overcome the resistance.

For a systematic investigation of the last-mentioned group of phenomena the apparatus was arranged as shown in fig. 6.



It is essentially a Wheatstone's bridge arrangement, the proportional coils used being 1 and 1,000 ohms. A tangent galvanometer of ·9 ohm resistance is inserted in the same arm as the carbon balance, and a box of resistance coils R is placed between the battery (which consists of eight or ten Leclanché cells) and the bridge. The arrangement admits of the measurement of the resistance of the carbon when traversed by strong currents, the strength of which can be regulated by the resistance box R, and measured by the tangent galvanometer G. A given weight being in the scale-pan, known currents of gradually increasing strength were passed through the carbon. The resistance was measured, 1st, while the known current was passing; 2nd, with a very small current obtained by unplugging 10,000 ohms in the resistance box. As the currents increased in strength, the resistance measured with the very small current gradually diminished until a point was reached—and the lighter the weight the sooner this was the case—at which the resistance suddenly became infinity. The measurements are given in Table IX, the currents varying from ·01 to ·7 ampère, and the pressures from ·05 grm. to 5 grms.

Table IX.

R_1 = Resistance with current in first column of table.
R_2 = Resistance with very weak current.

Current. Ampère.	Pressure.							
	·05 grm.		·5 grm.		2·5 grms.		5 grms.	
	R_1.	R_2.	R_1.	R_2.	R_1.	R_2.	R_1.	R_2.
·01	38	62	10·2	10·3	4·8	4·8	2·60	2·6
·05	21	38	9·7	10·1	4·6	4·7	2·54	2·6
·1	Inf.	..	8·1	9·3	4·4	4·7	2·37	2·5
·2	..	..	6·3	8·4	4·0	4·5	2·23	2·4
·3	..	..	Inf.	..	3·7	4·3	2·11	2·3
·4	..	..	..	..	3·6	4·2	2·01	2·3
·5	..	..	..	..	Inf.	..	1·95	2·2
·6	..	.	..	..	..	..	1·90	2·2
·7	..	..	..	..	..	..	Inf.	..

10. *Experiments with Metallic Contacts.*

For the sake of comparison a few experiments were made with metals. Metallic loose contacts were found to be even more uncertain in their action than those of carbon, and to obtain a really fair average of results a very great number of measurements would be required. Under precisely the same conditions as nearly as they could be reproduced, the results obtained were frequently very different. Nevertheless it has been possible to bring out with sufficient distinctness some remarkable peculiarities in the behaviour of metallic contacts.

The metal principally used was bismuth, which was selected on account of its high specific resistance; but a few experiments were also made with copper and platinum. The cylinders of bismuth and copper were nearly the same diameter as those of the carbon previously used—6 millims. The platinum was prepared by soldering wires of that metal upon cylinders of brass. The balance already described and shown at fig. 1 was used in the same manner as before.

11. *Permanent Fall of Resistance caused by Current.*

The first unexpected phenomenon which attracted notice in working with bismuth was the very great permanent fall in the resistance which was produced by an ordinary current with light pressures. Using a single Leclanché cell, the lightest contact—the pressure being certainly less than ·01 grm.—failed to give a higher resistance than

9 ohms; but with 400 ohms inserted between the battery and bridge, a pressure of ·35 grm. gave a resistance which on different occasions varied between 73 and 110 ohms. When once, however, the resistance has been reduced by a strong current, the changes which occur when feebler currents are used are comparatively small—unless, of course, the points of contact are first separated. Of the nature of such changes I shall say more presently. The following table gives a few measurements, using one Leclanché cell: (1) with a resistance of 300 ohms between the cell and the bridge; (2) without the 300 ohms. It will be understood that the bismuth cylinders were separated from each other every time the weights were changed. The effect in question is most marked with small pressures; with comparatively high pressures it quite disappears.

Table X.

Pressure.	Resistance of bismuth contacts: with 300 ohms between battery and bridge.	Resistance of bismuth contacts: when the 300 ohms is removed.
Grms.	Ohms.	Ohms.
·5	60	1·9
2·5	37·2	1·4
5	5·4	1·7
7·5	7·7	2
10	·8	·8
15	·6	·5
25	·14	·14

12. *Adhesion of Contacts due to Current.*

This phenomenon is accompanied by another. After the passage of the current which has caused the resistance to be so remarkably diminished, the bismuth cylinders are found to adhere to each other, a quite appreciable force being necessary to separate them. This effect, which is common to all metals, has been thoroughly investigated by Mr. Stroh, a description of whose experiments is given in the "Journal of the Society of Telegraph Engineers," vol. xix, page 182. I shall, therefore, say nothing more about it, except that I have succeeded in causing two pieces of very thin platinum wire to stick to each other with a current of about a milliampère. Mr. Stroh attributes the effect to fusion.

13. *Effect of Variation of Current after a strong Current has passed through the Contact.*

A third apparent anomaly is the following: a *small* weight being in

the scale-pan, let a weak current be caused to pass through the points of contact; the resistance will be found to be high. Let a strong current be passed, and the resistance will be greatly diminished. Once more let the current be reduced to its original strength; the resistance will not rise to its original value; it will not, in fact, rise at all; it will be still further diminished. The following table gives examples of this experiment with pressures of ·5, 1, and 5 grms.

Table XI.

Current. Ampère.	Resistance of bismuth contacts with pressure of		
	·5 grm.	1 grm.	5 grms.
·01	8	2·12	1·50
·5	1·27	1·27	1·22
·01	1·22	1·21	1·22
·5	1·27	1·26	1·22
·01	1·22	1·21	1·22

In the last case there is no change after the first fall of resistance. Table XII further illustrates the effect with greater variations of current and of weight, but in the experiments to which it relates the strongest current was used first. As before, the bismuth cylinders were separated only when the weights were changed.

Table XII.—Experiments with Clean Bismuth.

Current. Ampère.	Resistance of contacts with pressure of				
	·25 grm.	·35 grm.	·5 grm.	1 grm.	2 grms.
·5	1·52	1·39	1·55	1·33	1·35
·3	1·40	1·35	1·49	1·30	1·33
·1	1·37	1·33	1·45	1·28	1·32
·05	1·35	1·33	1·44	1·28	1·32
·01	1·18	1·32	1·41	1·28	1·32
·05	1·55	1·33	1·46	1·29	1·32
·1	1·72	1·34	1·47	1·29	1·33
·3	1·79	1·39	1·55	1·31	1·34
·5	1·49	1·43	1·61	1·33	1·35

In order that this experiment may be successful, it is necessary that the surfaces in contact should be absolutely clean. Unless they are scraped immediately before the measurements are made, the opposite

effect may result: that is (after the first great fall of resistance), diminished current may produce increased resistance. Table XIII gives the measurements, with the same currents as before, when the bismuth was first rubbed with the finger, instead of being scraped as had been done in the previous experiments.

Table XIII.—Experiments after Rubbing the Bismuth with the Finger.

Current. Ampère.	Resistance of contact with pressure of		
	·35 grm.	·5 grm.	1 grm.
·5	1·30	1·30	1·22
·3	1·75	1·46	1·28
·1	1·92	1·69	1·29
·05	1·83	1·92	1·29
·01	1·95	2·29	1·29
·05	1·89	2·07	1·28
·1	1·87	1·98	1·28
·3	1·77	1·72	1·27
·5	1·46	1·45	1·27

I think these effects admit of a simple explanation. When the surfaces of the bismuth are clean, contact takes place entirely through the metal. The current heats the metal at the points of contact to an extent which depends upon the current strength; and the resistance, in accordance with the general law, increases with the temperature: strong currents will, therefore, give higher resistance than weak ones. When, on the other hand, the surface is not clean, a film of oxide or some foreign substance is interposed, the resistance of which, like that of carbon, is higher with a weak current than with a strong one.

It is probable that similar effects occur with metals of lower specific resistance than that of bismuth; but their observation is very difficult, and requires more delicate apparatus than that at my disposal. Thus, of forty measurements made with platinum contacts, nineteen results were favourable to the theory, ten adverse to it, and eleven neutral. With copper the indications were even more uncertain.

14. *Effects on Resistance of Varying Pressure with Constant Current.*

The resistance of bismuth contacts at various pressures was measured with fixed currents of ·1, ·01, and ·001 ampère, the method used being the same as in the case of carbon. The mean of several series of such measurements is given in Table XIV, which reveals the existence of a more or less definite law, though the results of the experiments did not agree very closely among themselves.

Table XIV.—Effect of Pressure upon Resistance of Bismuth Contacts with Constant Currents.

Pressure. Grms.	Resistance of contacts with current of		
	·1 ampère.	·01 ampère.	·001 ampère.
·05	5	20·82	182
·1	2	16·92	143·3
·15	2·5	14·60	97·8
·2	3·9	12·60	46
·25	2·6	10·05	41·6
·3	2·4	6·35	16·9
·35	2·1	4·12	21·6
·4	1·9	2·37	30·6
·45	1·5	1·60	18·3
·5	1·45	1·47	3·8
1	·95	1·35	
1·5	·85	·70	
2	·90	·62	
2·5	·70	·30	
3	·50	·15	
3·5	·55	·07	
4	·35	·05	
4·5	·25		
5	·15		

15. *Permanent Reduction of Resistance due to Pressure.*

Repeating with bismuth the experiments already described in the case of carbon, it was clear that the diminution of resistance effected by pressure is generally of a permanent nature, continuing to a great extent after the pressure has been removed (so long as the points of contact remain undisturbed), and thus reversing the case of carbon. With strong currents the variations of resistance were so uncertain and irregular that accurate measurements were impossible; but with a resistance of 300 ohms between the battery and the bridge the effect is very clearly marked. The results of the experiments thus made are tabulated below.

Table XV.

Pressure. Grms.	Resistance of bismuth contacts.					
	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	10·9	87·5	16·3	14·5	25·5	62
8	1·3	1·7	·5	1	1·1	1·8
·5	1·5	1·7	·5	4·5	5·5	15

The points of contact were, of course, changed for each series of observations, and it should be noted that in the case of the last three (which were not made on the same day as the others) I was less successful in arranging the lever so as to secure perfect freedom from oscillation when the weight was changed.

16. *Summary.*

The following appear to be the most important results of the investigation.

(1.) *Carbon Contacts.*

Changes of pressure produce proportionately greater changes of resistance when the initial pressure is small than when it is great.

Changes of pressure produce proportionately greater changes of resistance with weak currents than with strong currents.

Changes of current produce proportionately greater changes of resistance with small currents than with large currents; and with light pressures than with heavy pressures.

When the resistance of a carbon contact has been reduced by an increase of pressure, it will, on the removal of the added pressure, rise to approximately its original value.

The passage of a current whose strength does not exceed a certain limit, depending upon the pressure, causes a permanent diminution in the resistance, and the stronger the current the greater will be such diminution.

When the strength of the current exceeds a certain limit the resistance is greatly and permanently increased. The greater the pressure the higher will be such limit.

Unless special means are adopted for preserving a constant current, the fall in the resistance which attends increased pressure is greater than that which is due to increased pressure alone; being partly due also to increased strength of current.

It is not proved that the diminished resistance which follows an increase of current is an effect of temperature.

(2.) *Metallic Contacts.*

In the case of bismuth and probably of other metals—

With a given pressure the weaker the current the higher will be the resistance. This effect is most marked when the pressure is small.

Increase in the strength of the current is accompanied by a fall in the resistance, and if the current be again reduced to its original strength, the resulting change in the resistance will be small, and it will in no case return to its original value.

Diminution in the strength of the current is followed by a small

fall in the resistance if the metal is clean, and by a small rise in the resistance if the metal is not clean.

Increased pressure produces a greater fall in the resistance with small pressures than with great pressures, and with weak currents than with strong currents.

The resistance after having been reduced by increased pressure does not return to its original value when the added pressure is removed.

17. *Reasons for the Superiority of Carbon over Metal in the Microphone.*

The above observations may, perhaps, throw some light on a matter which has never hitherto been fully explained. Why does carbon give far better results than any metal when used in the microphone? It seems to me that this question may be answered without much difficulty. The mere fact that a current causes delicately adjusted metal contacts to adhere to each other is sufficient to account for the superior efficiency of carbon. A metal microphone might, indeed, be used to transmit the pitch of a sound, provided that its vibrations were sufficiently powerful to cause actual separation of the points of contact. The fundamental tone might, in this way, be conveyed, but it is clear that the minute superimposed vibrations, due to the upper partials, upon which depends the distinctive character of a particular sound, would be very imperfectly represented if not entirely lost.

In addition to this phenomenon of adhesion, and probably connected with it, are the facts that metallic contacts, unlike those of carbon, do not even approximately recover their original resistance when once it has been reduced by increased pressure or increased current, unless, indeed, complete separation occurs: and even the initial effect of pressure upon resistance is (except with very weak currents), in general, much more marked with carbon than with metals.

Lastly, it is to be noticed that in the case of carbon, pressure and current act in consonance with each other: pressure diminishes the resistance, and in so doing increases the strength of the current; and the current thus strengthened effects a further diminution in the resistance. In the case of metals, on the other hand (or at least in the case of clean bismuth) pressure and current tend to produce opposite effects. The resistance is diminished by pressure and the current consequently strengthened, but by reason of the increased strength of current, the resistance is *higher* than it would have been if the current had remained unchanged. The effect of this antagonism is not very great, but it seems to give a material advantage to carbon.*

* In April, 1882, I communicated this observation to Mr. Preece, who referred to it in a paper, read at the Southampton Meeting of the British Association, on "Recent Progress in Telephony."

II. "On the Affinities of Thylacoleo." By Professor OWEN, C.B., F.R.S., &c. Received January 25, 1883.

(Abstract.)

Since the communication of the paper "On Thylacoleo," in the "Philosophical Transactions" for 1871, further explorations of the caves and breccia-fissures in Wellington Valley, New South Wales, have been made, by a grant for that purpose from the Legislature of the Colony, and carried out by E. B. Ramsay, Esq., F.L.S., Curator of the Museum of Natural History, Sydney. The present paper treats of the fossils contributing to the further restoration of the great carnivorous Marsupial (*Thylacoleo carnifex*, Ow.). They exemplify the entire dentition *in situ* of the upper and lower jaws of a mature individual: the bones of the fore-limb, of which those of the antibrachium and the ungual phalanges are described, are compared with those of other Marsupials, and of placental, especially feline, *Carnivora*. An entire lower jaw with the articular condyles adds to the grounds for determination of the habits and affinities of the extinct Marsupial.

Figures of these fossils of the natural size accompany the paper.

III. "Preliminary Note on a Theory of Magnetism based upon New Experimental Researches." By Professor D. E. HUGHES, F.R.S. Received January 27, 1883.

In the year 1879* I communicated to the Royal Society a paper "On an Induction Currents Balance and Experimental Researches made therewith." I continued my researches into the molecular construction of metallic bodies, and communicated the results then obtained in three separate papers† bearing upon molecular magnetism.

To investigate the molecular construction of magnets required again special forms of apparatus, and I have since been engaged upon these, and the researches which they have enabled me to follow.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

* "Proc. Roy. Soc.," vol. 29, p. 56, 1879.

† "Proc. Roy. Soc.," vol. 31, p. 525; vol. 32, pp. 25, 213, 1881.

2. That each molecule, or its polarity, can be rotated in either direction upon its axis by torsion, stress, or by physical forces such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules, or their polarities, arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules or their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in this direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism, we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

The experimental evidences of the above theory are extremely numerous, and appear so conclusive, that I have ventured upon formulating the results in the above theory.

I hope in a few weeks to bring before the Royal Society the experimental evidence which has led me to the conclusions I have named; conclusions which have not been arrived at hastily, but from a long series of research upon the molecular construction of magnetism now extending over several years.

February 8, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Paper were read :—

"Note on Terrestrial Radiation." By JOHN TYNDALL, F.R.S. Received February 5, 1883.

On Hind Head, a fine moorland plateau about three miles from Haslemere, with an elevation of 900 feet above the sea, I have recently erected a small iron hut, which forms, not only a place of rest, but an extremely suitable station for meteorological observations. Here, since the beginning of last November, I have continued to record from time to time the temperature of the earth's surface as compared with that of the air above the surface. My object was to apply, if possible, the results which my experiments had established regarding the action of aqueous vapour upon radiant heat.

Two stout poles about 6 feet high were firmly fixed in the earth 8 feet asunder. From one pole to the other was stretched a string, from the centre of which the air thermometer was suspended. Its bulb was 4 feet above the earth. The surface thermometer was placed upon a layer of cotton wool, on a spot cleared of heather, which thickly covered the rest of the ground. The outlook from the thermometers was free and extensive; with the exception of the iron hut just referred to, there was no house near, the hut being about 50 yards distant from the thermometers.

On November 11th, at 5.45 P.M., these were placed in position, and observed from time to time afterwards. Here are the results :—

6 P.M.	......	Air 36° Fahr.	......	Wool 26° Fahr.
8.10 ,,	......	,, 36	......	,, 25
9.15 ,,	......	,, 36	......	,, 25

air almost dead calm, sky clear, and stars shining.

November 12th, the wind had veered to the east, and was rather strong. The thermometers, exposed at 5 P.M., yielded the following results :—

5.15 P.M.	....	Air 38°	....	Wool 33°
5.45 ,,	....	,, 38	....	,, 34
6.45 ,,	....	,, 38	....	,, 35
9 ,,	....	,, 39	....	,, 36

During the first and last of these observations the sky was entirely overcast, during the other two a few stars were dimly visible.

On November 13th, 25th, and 26th, observations were also made, but they presented nothing remarkable.

It was otherwise, however, on December 10th. On the morning of that day the temperature was very low, snow a foot deep covered the heather, while there was a very light movement of the air from the north-east. Assuming aqueous vapour to play the part that I have ascribed to it, the conditions were exactly such as would entitle us on *à priori* grounds to expect a considerable waste of the earth's heat. At 8.5 A.M. the thermometers were placed in position, having left the hut at a common temperature of 35°. The cotton wool on which the surface thermometer was laid was of the same temperature. A single minute's exposure sufficed to establish a difference of 5° between the two thermometers. The following observations were then made:—

8.10 A.M.	Air 29°	Wool 16°
8.15 „	„ 29	„ 12

Thus, in ten minutes, a difference of no less than 17° had established itself between the two thermometers.

Up to this time the sun was invisible: a dense dark cloud, resting on the opposite ridge of Blackdown, virtually retarded his rising.

8.20 A.M.	Air 27°	Wool 12°
8.30 „	„ 26	„ 11
8.40 „	„ 26	„ 10
8.45 „	„ 27	„ 11
8.50 „	„ 29	„ 11

During the last two observations, the newly risen sun shone upon the air thermometer. As the day advanced the difference between air and wool became gradually less. From 18° at 8.50 A.M., it had sunk at 9.25 to 15°, at 9.50 to 13°, while at 10.25, the sun being unclouded at the time, the difference was 11°; the air at that hour being 31° and the wool 20°.

In the celebrated experiments of Patrick Wilson, the greatest difference observed between a surface of snow and the air 2 feet above the snow, was 16°; while the greatest difference noticed by Wells during his long continued observations, fell short of this amount. Had Wilson employed swandown or cotton wool, and had he placed his thermometer 4 feet instead of 2 feet above the surface, his difference would probably have surpassed mine, for his temperatures were much lower than those observed by me. There is, however, considerable similarity in the conditions under which we operated. Snow in both cases was on the ground, and with him, there was a light movement of the air from the east, while with me the motion

was from the north-east. The great differences of temperature between earth and air which both his observations and mine reveal, are due to a common cause, namely, the withdrawal of the check to terrestrial radiation which is imposed by the presence of aqueous vapour.

Let us now compare these results with others obtained at a time of extreme atmospheric serenity, when the air was almost a dead calm, and the sky without a cloud. At 3.30 P.M., January 16th, the thermometers were placed in position, and observed afterwards with the following results:—

3.40 P.M.	Air 43°	Wool 37°
3.50 „	„ 42	„ 35
4 „	„ 41	„ 35
4.15 „	„ 40	„ 34
4.30 „	„ 38	„ 32
5 „	„ 37	„ 28
5.30 „	„ 37	„ 30
6 „	„ 36	„ 32

These observations, and especially the last of them, merit our attention. There was no visible impediment to terrestrial radiation. The sky was extremely clear, the moon was shining, Orion, the Pleiades, Charles' Wain, including the small companion star at the bend of the shaft, the north star, and many others, were clearly visible. On no previous occasion during these observations had I seen the firmament purer; and still, under these favourable conditions, the difference between air and wool at 6 P.M. was only 4°, or less than one-fourth of that observed on the morning of the 10th of December.

We have here, I submit, a very striking illustration of the action of that invisible constituent of the atmosphere, to the influence of which I drew attention more than twenty-two years ago. On the 10th of December the wind was light from the north-east, with a low temperature. On the 16th of January it was very light from the south-west, with a higher temperature. The one was a dry air, the other was a humid air; the latter, therefore, though of great optical transparency, proved competent to arrest the invisible heat of the earth.

The variations in the temperatures of the wool recorded in the last column of figures are, moreover, not without a cause. The advance of temperature from 28° at 5 P.M. to 32° at 6 P.M., is not to be accounted for by any visible change in the atmosphere, or by any alteration in the motion of the air. The advance was due to the intrusion at 6 P.M. of an invisible screen between the earth and firmament.

As the night advanced, the serenity of the air, became, if possible, more perfect, and the observations were continued with the following results:—

6.30 P.M.	Air 36°	Wool 31°
7 ,, ...	,, 36	,, 28
7·30 ,, ...	,, 35½	,, 28
8 ,, ...	,, 35	,, 26
8.30 ,, ...	,, 34	,, 25
9 ,, ...	,, 35	,, 27
10 ,, ...	,, 35	,, 28
10.30 ,, ...	,, 35	,, 29

After this last observation, my notes contain the remark, "Atmosphere exquisitely clear. From zenith to horizon cloudless all round."

Here, again, the difference of 4° between the temperature of the wool at 8.30 P.M., and its temperature at 10.30 P.M., is not to be referred to any sensible change in the condition of the atmosphere.

The observations were continued on January 17th, 23rd, 24th, 25th, and 30th; but I will confine myself to the results obtained on the evening of the day last mentioned. The thermometers were exposed at 6.45 P.M., and by aid of a lamp read off from time to time afterwards.

7.15 P.M.	Air 32°	Wool 26°
8 ,, ...	,, 31	,, 26
9.30 ,, ...	,, 31	,, 27

During these observations the atmosphere was very serene. There was no moon, but the firmament was powdered with stars. The serenity, however, had been preceded by heavy rain, which doubtless had left the atmosphere charged with aqueous vapour. The movement of the air was from the south-west and light. Here again, with an atmosphere at least as clear as that on December 10th, the difference between air and wool did not amount to one-fourth of that observed on the latter occasion.

The results obtained on February 3rd were corroborative. The thermometers were exposed at 6.15 P.M.

7.15 P.M.	Air 34°	Wool 28°
8.25 ,, ...	,, 34	,, 30

Here again, the difference between air and wool is only 4 degrees, although the sky was cloudless, and the stars were bright. The movement of the air was from the south-west and light.

On the forenoon of this day there had been a heavy and persistent rain storm. Heavy rain and high wind also occurred on the night following. The serene interval during which the observations were made lay, therefore, between the two storms. Doubtless the gap was well filled with pure aqueous vapour.

Further observations were made in considerable numbers, but they

need not here be dwelt upon, my object being to illustrate a principle rather than to add to the multitudinous records of meteorology. It will be sufficient to say, that with atmospheric conditions sensibly alike, the waste of heat from the earth varies from day to day; a result due to the action of a body which escapes the sense of vision. It is hardly necessary for me to repeat here my references to the observations of Leslie, Hennessey, and others, which revealed variations in the earth's emission for which the observers could not account. A close inspection of the observations of the late Principal Forbes on the Faulhorn, proves, I think, that the action of aqueous vapour came there into play, and his detection of this action, while unacquainted with its cause, is in my opinion a cogent proof of the accuracy of his work as a meteorologist.

POSTSCRIPT.

In the "Philosophical Transactions" for 1882, Part I, p. 348, I refer to certain experiments executed by Professor Soret of Geneva. My friend has recently drawn my attention to a communication made by him to the French Association for the Advancement of Science, in 1872. It gives me great pleasure to cite here the conclusions at which he has arrived.

"The influence of humidity is shown by the whole of the observations; and it may be stated generally, that, other circumstances being equal, the greater the tension of aqueous vapour, the less intense is the radiation.

"In winter, when the air is drier, the radiation is much more intense than in summer, for the same height of the sun above the horizon.

"On several occasions a more intense radiation has been observed in dry than in humid weather, although the atmosphere was incontestably purer and more transparent in the second case than in the first.

"The maximum intensity of radiation, particularly in the summer, corresponds habitually to days exceptionally cold and dry."

Such are the results of experiments, executed by a most excellent observer, on the radiation of the sun. They apply word for word to terrestrial radiation. They are, moreover, in complete harmony with the results, published by General Strachey in the "Philosophical Magazine" for 1866. Meteorologists will not, I trust, be offended with me if I say that from such outsiders, fresh to the work and equipped with the necessary physical knowledge, they may expect efficient aid towards introducing order and causality among their valuable observations.

February 15, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Some Experiments on Metallic Reflection. No. III. On the Amount of Light Reflected by Metallic Surfaces." By Sir JOHN CONROY, Bart., M.A. Communicated by Professor STOKES, Sec. R.S. Received November 1, 1882.

As far as I am aware the only experiments that have been made on the amount of radiant energy reflected by metallic surfaces at different angles are those of Potter ("Edinburgh Journal," vol. iii, 278) and Jamin ("Ann. de Chim. et de Phys." [3], xix, 296) for light, and for radiant heat, those of Forbes ("Phil. Mag.," [3], viii, 246) and of MM. De la Provostaye and Desains ("Ann. de Chim. et de Phys." [3], xxx, 276.

Potter used a Bouguer's photometer; the transparent screen being made of white paper, behind which two lamps were placed, the light from one of which always fell directly upon one half of the screen, whilst that from the other either fell directly upon the screen, or after reflection from the metallic plate. The observations were made by measuring the distance at which the first lamp had to be placed in order that both halves of the screen should appear equally bright.

The two illuminated portions of the screen were not actually in contact, being separated by a dark shadow, an arrangement which, to a certain extent at least, must have interfered with the accuracy of the determinations. The mirror having been placed close to the lamp, the light incident upon its surface must have been very divergent, and as, in addition, the angle at which it was placed could not, owing to the construction of the apparatus, have been very accurately determined, the values of the angles of incidence can only be considered as approximations.

In M. Jamin's experiments the reflecting surface was half glass and half metal, the line of separation being vertical, and the incident light polarised in a plane 45° to this direction; the reflected light was examined with a double image prism which was rotated until one of the images due to the light reflected by the metal, and one of those

due to the light reflected by the glass appeared equally bright, *i.e.*, till the ordinary image of the light reflected by one-half of the mirror was equal to the extraordinary image of the light reflected by the other half.

The refractive index of the glass being known, the amount of light reflected by it at any angle could be calculated by Fresnel's formulæ, and thus the percentage of light reflected by the metal determined.

The numbers obtained by M. Jamin agree well with those deduced by calculation from Cauchy's theory, and also with those experimentally determined by MM. De la Provostaye and Desains, by means of the thermopile. Unfortunately, however, the experiments on this point made by these two eminent French physicists were not very numerous, and the method used by M. Jamin has been described by M. Verdet ("Leçons d'Optique Physique," ii, 546) as "Un procédé indirect qui n'est pas susceptible d'une grande perfection."

Under these circumstances I trust that some experiments which I have recently made on the subject may be thought worthy of publication, although, owing mainly to the difficulties inseparable from all photometric determinations, the observations are not as concordant as could be wished.

The method used was essentially that of Potter, the experiments being made by comparing photometrically the amount of light reflected by a polished metallic surface at different angles with that which fell directly on the photometer when the reflecting surface was removed.

This method is, of course, only applicable to the white metals, and in order to obtain anything like accurate results the mean of a considerable number of observations must be taken.

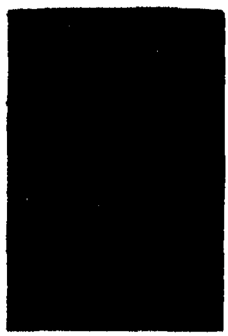
Two similar paraffine lamps, with flat wicks, were used, one arranged to slide along a horizontal board about two metres long, to which a scale divided into millimetres was attached, and the other supported by a metal ring fastened to one of the arms of a Babinet's goniometer.

An endless cord, which passed round a pulley with a handle at one end of the board, enabled the first-mentioned lamp to be moved and placed at different distances from the photometer.

It was originally intended to use a Bunsen's disk, but it was found that owing to the small size of the beam of reflected light, it was not possible to make satisfactory measurements with it, and after various arrangements had been tried, a modification of Ritchie's photometer was finally adopted. Two pieces of white paper were so placed that whilst both were visible to the observer, one being slightly in front of the other and overlapping it to a small extent, each received light from one only of the lamps, and when equally illuminated the edge of the front paper vanished. Two triangular blocks of wood 4 centims.

high, were screwed to a rectangular board about 15 centims. by 10 centims., in the position shown in the figure, and pieces of white paper, 3 centims. by 3 centims., held against the hypotenuse of each of these triangular prisms by india-rubber bands. The whole arrangement was enclosed in a box with three apertures, which was painted, both internally and externally, a dead black, and was placed at the end of the horizontal board.

The light from the sliding lamp entered by the left-hand aperture, whilst that which fell directly on the paper, or after reflection from the metallic surface, entered to the right-hand one; the third aperture allowed the papers to be seen by the observer, who was at about 6 decims. from it.



¼th the actual size.

The goniometer was fixed on the other side of the photometer, with its vertical axis in the prolongation of the median line of the board, and had a vertical stage, to which the reflecting plate could be readily fastened. The second lamp, which, as before mentioned, was carried by a plate attached to one of the arms of the goniometer, was always placed with its flame edgewise, *i.e.*, radially, and the beam of light limited by means of a diaphragm with an aperture 25 millims. high and 5 millims. wide, placed at a distance of 23 centims. from the centre of the flame and between it and the axis of the instrument. The sliding lamp was placed with the flat side of the flame towards the photometer.

The experiments were made by first turning the goniometer until the light fell on the paper of the photometer; the position of the sliding lamp was then altered until both papers appeared equally illuminated, and the distance of the lamp from the central line of the photometer observed. Four such observations were made, in the first and third the sliding lamp being placed too near the photometer and the distance increased, and in the second and fourth the lamp placed too far off and the distance diminished until the illumination of the two papers becomes equal.

The metal mirror was then clamped to the stage, which had previously been carefully adjusted, in order that the surface of the mirror might be vertical and in the axis of the instrument, and the whole instrument rotated on its outer axis until the reflected beam fell on the photometer. Four readings were then made, as has already been described, the mirror removed, and four more readings made of the intensity of the light emitted by the lamp; the mirror replaced, the angle of incidence altered, and four more readings made, and so on.

It was found necessary to make the observations in this way, as although the two lamps were trimmed as nearly as possible alike, considerable fluctuations in their relative intensity not unfrequently occurred, and in order to diminish as far as possible this source of error, the mean of eight observations, four before and four after the measurement of the intensity of the reflected light, was taken as the true intensity of the light incident upon the mirror.

The metal surfaces used measured about 8 centims. by 5 centims., were accurately plane, and had all been polished with putty powder, it having been previously ascertained ("*Proc. Roy. Soc.*," vol. 31, p. 486) that the optical constants for metallic surfaces depend to a certain extent upon the nature of the substance with which they have been polished.

It had been originally intended to use light polarised in and perpendicularly to the plane of incidence, and to determine the ratio of the reflected to the incident light in either case, but it was found impossible to make any satisfactory measurements, owing to the great loss of light. The intensity of the light could have been somewhat increased by the use of a larger Nicol, but it seemed so very doubtful whether sufficient light would be thus obtained, that it was thought best to abandon the use of polarised light. Ordinary light being equivalent to two beams of light of equal intensity polarised at right angles to each other, if the total amount reflected at any angle, and the ratio of the intensities of the light polarised in and perpendicularly to the plane of incidence when light polarised at an angle of 45° with that plane is incident upon the surface of the plate at the same angle, are known, the reflective power of the plate for light polarised in and perpendicularly to the plane of incidence can of course be readily calculated.

The table gives a series of measurements made with a silver plate. The numbers in the first column are the distances in centimetres of the sliding lamp from the photometer when the light of the other lamp fell direct on the paper, and those in the third when the light was reflected by the mirror. The means of these observations are contained in the second and fourth columns, the angles of incidence in the fifth column, and the ratios of the reflected to the incident light, the latter being taken as 100, in the sixth column. As the intensity

	Mean.							
97·5, 98·3, 96·1, 97·2								
	97·4	116·0, 115·2, 114·5, 113·3	114·7	....	10°	....	72·11
96·7, 98·6, 97·2, 97·9								
	97·5	115·9, 113·3, 113·7, 114·4	114·3	....	10°	....	73·10
96·5, 98·0, 96·1, 99·0								
	98·0	114·0, 113·3, 114·7, 115·7	114·4	....	20°	....	73·38
97·8, 99·5, 98·7, 99·1								
	98·6	115·2, 114·6, 112·3, 114·5	114·1	....	30°	....	74·67
98·7, 98·5, 98·1, 99·7								
	98·6	114·3, 114·9, 114·0, 115·0	114·5	....	40°	....	74·15
98·0, 98·7, 97·5, 99·8								
	98·2	115·0, 113·5, 114·0, 112·8	113·8	....	50°	....	74·46
97·6, 98·2, 97·9, 98·1								
	97·7	111·6, 112·9, 112·3, 111·8	112·1	....	60°	....	75·94
96·9, 98·6, 96·5, 98·0								
	97·1	111·3, 111·5, 110·7, 111·6	111·3	....	65°	....	76·11
96·0, 98·0, 95·6, 97·5								
	97·1	110·0, 110·7, 109·2, 111·2	110·3	....	70°	....	77·40
98·0, 98·3, 97·2, 99·1								
	98·3	110·1, 109·5, 110·8, 109·3	109·9	....	75°	....	80·00
97·3, 97·7, 99·1, 99·8								
	98·5	109·1, 111·5, 109·7, 111·5	110·4	....	77°	....	79·60
96·9, 99·6, 97·5, 100·2								

of light varies inversely as the square of the distance from the source, the percentage reflected by the plate is obtained by dividing the numbers contained in the second column by those in the fourth, squaring, and multiplying by 100.

Two other series of observations were made with the silver plate, the measurements being about as concordant as those given above. Table I contains the results of these three series and their mean.

Table I.

Angle of Incidence.	Percentage amount of Light Reflected by the Silver Mirror.			
°	A.	B.	C.	Mean.
10	72·60	64·79	70·20	70·05
20	73·38	65·77	71·02	70·06
30	74·67	65·80	73·57	71·35
40	74·15	66·25	72·20	70·87
50	74·46	69·37	73·65	72·49
60	75·94	71·46	75·18	74·19
65	76·11	70·45	74·17	73·58
70	77·49	72·30	74·09	74·63
75	80·0	{76·50 75·92}	76·58	77·25
77	79·60	..	..	79·60
80	..	{80·95 81·60}	81·03	81·19

Note.—The mirror was slightly tarnished when the B series of measurements were made.

Similar measurements were made with steel, tin, and speculum metal plates. The results are given in Tables II, III, and IV.

Table II.

Angle of Incidence.	Percentage amount of Light reflected by Steel Mirror.			
°	A.	B.	C.	Mean.
10	53·53	54·66	54·97	54·38
20	55·18	54·42	56·58	55·39
30	55·38	53·77	55·64	54·93
40	56·04	54·80	56·01	55·62
50	56·10	56·58	57·54	56·74
60	58·50	56·72	57·48	57·63
65	59·23	57·39	58·50	58·37
70	57·22	57·59	59·45	58·09
75	59·16	56·22	60·69	58·69
80	65·23	61·23	64·22	63·56

Table III.

Angle of Incidence.	Percentage amount of Light reflected by Tin Mirror.			
°	A.	B.	C.	Mean.
10	39·54	40·34	39·39	39·76
20	39·69	41·01	40·14	40·28
30	42·65	45·48	45·02	44·38
40	43·19	43·23	45·91	44·11
50	46·25	45·31	50·80	47·48
60	49·63	49·77	52·40	50·60
65	51·24	51·47	54·25	52·32
70	52·79	54·95	57·17	54·97
75	57·89	58·92	59·73	58·85
80	65·86	63·80	65·57	65·08

Table IV.

Angle of Incidence.	Percentage amount of Light reflected by Speculum Metal Mirror.			
°	A.	B.	C.	Mean.
10	63·83	67·11	67·45	66·13
20	65·51	67·29	67·84	66·88
30	65·77	67·96	66·87	66·87
40	66·93	68·49	66·35	67·26
50	67·35	67·37	67·07	67·26
60	66·97	66·99	66·98	66·32
65	65·71	66·49	67·39	66·53
70	66·70	67·64	68·60	67·65
75	65·68	68·80	67·82	67·43
80	71·72	69·86	68·94	70·17

The principal incidences and azimuths for the four mirrors were determined in the manner described in the paper on metallic reflection which has already been referred to ("Proc. Roy. Soc.," vol. 31, p. 486), a soda flame being used as the source of light. Four observations were made in each position of the retarding plate, two with the principal section of the polarising Nicol on the right, and two with it on the left of the plane of incidence. The means of several sets of eight observations each are given in Table V.

Table V.

	Principal Incidence.	Mean.		Principal Azimuth.	Mean.
Silver	74° 20′	74°·04′	....	39° 03′	39°·27′
	74 01		....	39 18	
	73 52		....	40 0	
Steel	76 49	76 ·48	....	28 12	27 ·53
	76 48		....	27 34	
Tin	75 0	74 ·17	....	32 02	31 ·26
	74 01		....	31 15	
	73 51		....	31 01	
Speculum Metal	75 39	75 ·35	....	33 23	33 ·12
	75 32		....	33 01	

The tables show that the amount of light reflected increases with the angle of incidence. In the case of speculum metal, however, after first increasing, the amount of light appears to diminish slightly, and after passing through a minimum at about 65° to increase again.

These results are not in accordance with the experiments of Potter or of M. Jamin. Potter found that the amount of light reflected diminished as the angle of incidence increased, being a maximum for perpendicular incidence; a result that was confirmed by the experiments of M. Jamin, who showed that at angles greater than any at which Potter had made observations, the amount of reflected light increased again.

The values of the principal incidences and azimuths given in Table V were used for calculating the amount of light which, according to Cauchy's theory, should have been reflected by the plates.

His formulæ are—

For light polarised in the plane of incidence

$$J^2 = \tan(\phi - 45):$$

For light polarised perpendicularly to the plane of incidence

$$I^2 = \tan(\chi - 45).$$

ϕ and χ are given by the equations—

$$(1) \qquad \cot\phi = \cos(2\epsilon - u)\sin\left(2 \text{ arc} \tan\frac{U}{\theta^2 \cos i}\right),$$

$$\cot\chi = \cos u \sin\left(2 \text{ arc} \tan\frac{\cos i}{U}\right).$$

θ and ϵ being two constants, and U and u two variables, determined by the relation :—

$$(2) \qquad \cot(2u - \epsilon) = \cot\epsilon \,.\, \cos\left(2 \text{ arc} \tan\frac{\sin i}{\theta}\right),$$

$$\theta^2 \sin 2\epsilon = U^2 \sin 2u.$$

At the angle of polarisation

$$u=2\beta, \qquad (\mathrm{U}=\sin \mathrm{B} \tan \mathrm{B}),$$

when β = the principal azimuth and B the principal incidence.

The forms which have been given to these expressions by MM. de la Provostaye and Desains ("Ann. de Chim." (3), 30, p. 279), are more convenient for calculation, and therefore were used.

$$\mathrm{J}^2=\frac{\theta^2+\cos^2 i-2\theta\cos\epsilon\cos i}{\theta^2+\cos^2 i+2\theta\cos\epsilon\cos i}.$$

$$\mathrm{I}^2=\frac{\theta^2\cos^2 i+1-2\theta\cos\epsilon\cos i}{\theta^2\cos^2 i+1+2\theta\cos\epsilon\cos i}.$$

These authors remark in their paper that it is generally sufficient to take $\epsilon=u$, and therefore $\theta=\mathrm{U}$,* and this was done in calculating out the intensities. The incident light having been unpolarised, half the sum of the intensities of the light polarised in, and perpendicularly to the plane of incidence $\{i.e., \frac{1}{2}(\mathrm{J}^2+\mathrm{I}^2)\}$ was taken as the theoretical intensity of the reflected light.

Amount of Light Reflected by the four Mirrors.

Angle of Incidence.	Silver.		Steel.	
	Observed.	Calculated.	Observed.	Calculated.
°				
20	70·06	80·97	55·39	59·19
40	70·87	80·84	55·62	58·92
60	74·19	80·24	57·63	57·66
80	81·19	83·68	63·56	60·71
	Tin.		Speculum Metal.	
20	40·28	60·55	66·88	66·85
40	44·11	60·41	67·26	66·62
60	50·60	60·01	66·32	65·64
80	65·08	66·98	70·17	69·60

The table shows that with the speculum metal mirror the observed and calculated intensities agree fairly well, but that such is not the

* According to Lundquist ("Pogg. Ann.," 152, p. 410), Jamin himself appears to have done so; in this case the formulæ (1) for determining ϕ and χ become respectively—

$$\cot\phi=\cos 2\beta\sin\left(2\arctan\frac{\cos \mathrm{B}}{\sin^2 \mathrm{B}\cos i}\right),$$

$$\cot\chi=\cos 2\beta\sin\left(2\arctan\frac{\cos \mathrm{B}\cos i}{\sin^2 \mathrm{B}}\right).$$

case with the other three mirrors. All four mirrors had appeared perfectly untarnished and bright when the observations were made, but as the silver, steel, and tin mirrors had been polished some months before they were used, it was thought possible that slight films might have formed on their surfaces, and that the difference in the calculated and observed results was due to this cause. The silver mirror was therefore returned to the maker, Mr. Hilger, to be repolished with putty power.

The amount of light reflected by it was determined the same day that it was received back, and it was found that its reflective power was slightly diminished for light incident upon its surface at angles of 20°, 40°, and 60°, and somewhat increased for light incident at 80°, the number being—

20°	......	68·54
40	......	69·01
60	......	72·65
80	......	85·96

These results agreeing fairly well with the means of the numerous observations which had previously been made, it was not thought necessary to make any further determinations, as it was clear that the difference between the observed and calculated values could not be due to a film on the surface of the mirror.

The surface of the silver mirror not being very good, it was again returned to the maker to be polished with rouge, that being stated to be the best material for polishing silver; the result was not very satisfactory, as the surface appeared less good than before—that this was really the case was confirmed by the reflective power of the mirror being diminished.

The results of three series of observations are recorded in table, and also two determinations of the values of the principal incidences and azimuths.

Angle of Incidence.	A.	B.	C.	Mean Value.
°				
10	58·69	63·01	61·38	61·03
20	61·02	63·95	63·74	62·90
30	62·48	64·80	66·21	64·49
40	64·27	66·57	66·95	65·93
50	65·24	67·82	68·50	67·19
55	65·71	68·19	69·89	67·93
60	66·71	70·19	70·98	69·29
65	68·88	70·54	70·11	69·84
70	68·95	72·77	72·71	71·48
75	70·87	73·43	77·26	73·85
80	77·16	78·92	83·15	79·74

Principal Incidence.		Principal Azimuth.
73·38	......	38·39
73·37	......	38·40

The theoretical intensities calculated by Cauchy's formulæ from the value of the principal incidence and principal azimuths are—

Incidence.		Observed.		Calculated.
20°	....	62·90	....	78·09
40	....	65·93	....	77·95
60	....	69·29	....	77·46
80	....	79·74	....	81·34

These numbers do not agree any better than those previously obtained, and it therefore seems necessary to assume that Cauchy's formulæ (at least in their simplified form) do not express the facts of the case, except, perhaps, for speculum metal.

The values for the intensity of the light calculated by the formulæ given by Professor James MacCullagh ("Collected Works," p. 133) decrease slowly up to a large angle of incidence, and then increase again just as is the case with the similar formulæ of Cauchy.

Professor Stokes suggested that very probably the discrepancy between the observed and calculated results was due to imperfect polish, as the differences were greater with the soft metals, silver and tin, to which it is more difficult to give a good polish than for the hard metals, steel and speculum metal, and also as the observed intensities fell short of the calculated ones at moderate incidences, whilst sometimes even exceeding them at high incidences, for which deficiencies of illumination due to defects of polish might possibly be expected to disappear.

Professor Stokes also suggested a method for examining the polish of the mirrors.

In accordance with his suggestion a cylindrical tinned iron canister, closed at one end, about 9 centims. in diameter and 27 centims. deep, was blackened internally and supported on a table at an angle of about 30° with the horizon, and with the lower edge of the open end about 4 centims. above the surface of the table, which was covered with a black cloth.

The table was placed out of doors, so that there might be plenty of light coming from all round, and the metal plates laid on the black cloth in front of the canister, so that an observer standing in front could see, by reflection, into the perfect darkness. If the polish were perfect, the surface of the metal plate would appear perfectly black; and if such should not be the case, the illumination of the plate would afford an estimate of the defect of polish.

The reflection in the plate was examined through a small hole in a

black screen, in order to prevent any light diffused from the observer's face being reflected by the plate into the canister, and thus destroying its perfect blackness.

Two of the plates were placed side by side and examined together, and in this way it was ascertained that their order of polish was, steel, speculum metal, silver, and tin; there being but little difference between the polish of the steel and the speculum metal, and a considerable difference between the speculum metal and the silver, and again between the silver and the tin.

The experiment was originally made with the silver mirror polished with putty powder, and was repeated after it had been polished with rouge; the order remained the same, but it was thought that there was a greater difference between the speculum metal and the silver in the latter case.

Professor Stokes also suggested that the surface of perfectly clean mercury would furnish a standard. Some mercury was, therefore, cleaned by being well shaken with pounded sugar, and then filtered three or four times through a cone of writing paper, with a small aperture at the apex. A small porcelain basin was blackened both internally and externally, and nearly filled with the clean mercury, and the reflection of the canister in it and in the plates compared. The reflection in the steel appeared quite as black as that in the mercury, whilst that in the speculum metal appeared slightly less black. In a preliminary experiment the steel was thought to be blacker than the mercury; the difference, if any, was, however, very slight, and was probably due to a thin film having formed on the surface of the mercury which, in that case, had only been cleaned by filtration through paper. The experiment was therefore repeated with mercury which had been cleaned as has already been described, but it was still found that, as compared with the mercury, the polish of the steel was sensibly perfect. The difference between the calculated and observed results for the steel at least cannot, therefore, be due to imperfect polish, and the discrepancy is almost too great to be accounted for by errors of observation, especially as the intensities actually observed increase with the incidence, whilst theoretically they ought to diminish, and then increase again.

The experiments show (unless there is some error due to the method of observation, and therefore common to all the determinations), that the amount of light reflected by silver, steel, and tin gradually increases with the angle of incidence; that with speculum metal after first increasing it diminishes slightly, and then increases again; that the results obtained by the method described in this paper are not in accordance with the experiments of Potter and M. Jamin, or with the values calculated by the formulæ of Cauchy and MacCullagh;

that with the silver and the tin mirrors the difference between theory and observation may be due to imperfect polish, but that such can hardly be the case with the steel.

Under these circumstances it appears desirable that a fresh series of observations should be made by some independent method, which would either confirm or disprove the results contained in this paper, and this I hope to attempt.

Received December 18, 1882.

All previous *determinations* of the *reflective* power of metals having been made in air, it appeared desirable to make some observations with the steel and speculum metal mirrors in water, the polish of these mirrors being satisfactory.

A glass trough about 7·6 centims. square and 3·7 centims. deep, was filled with distilled water and placed on the stage of the goniometer in such a position that the light from the lamp passed normally through two of its opposite sides, and then fell on the photometer. The distance of the sliding lamp was then altered till both papers appeared equally illuminated; four such readings were made, and then the mirror, which had been previously adjusted, placed in the trough and clamped at an angle of 45° with the incident light.

The trough was then so adjusted that the incident and emergent light passed normally through two of its adjacent sides, the goniometer turned until the light fell on the photometer, and the readings made in the usual way. The mirror was then removed and the intensity of the light which passed through the trough again determined.

The partial reflections from the glass sides of the trough, and the length of the path of the light in the water (the reflecting surface coinciding with the diagonal of the trough), being the same in both cases, the difference in the intensity of the light could only be due to the loss caused by reflection.

The light not being parallel, but forming a slightly divergent beam, the effect of introducing the trough of water between the source and the photometer was equivalent, in an optical sense, to slightly reducing the distance between them; it was therefore necessary that the light when falling directly on the photometer, and when doing so after reflection, should have traversed in both cases an equal thickness of water. This condition prevented observations being made at angles other than 45°.

The results of four observations with the speculum metal, and three with the steel mirrors, show that, as might have been anticipated, the percentage of light reflected was less than when the mirrors were in air; the numbers are—

	Speculum Metal.		Steel.
	58·73		
	56·13	......	44·74
	59·43	......	44·74
	57·82	......	44·20
Mean......	58·03	......	44·42

With the exception of the first determination made with the speculum metal mirror, which is the mean of eight, each observation is the mean of four readings.

The principal incidences and azimuths of the mirrors when in water were determined by the same method that had been used for ascertaining the values of these constants in air; the mirrors being surrounded by a cylindrical glass vessel filled with water.

	Speculum Metal.			Steel.	
	P.I.	P.A.		P.I.	P.A.
	72° 14′	32° 30′	....	72° 37′	27° 41′
	72 15	31 53	....	72 17	27 14
Mean..	72 14	32 11	....	72 27	27 27

These values were introduced into Cauchy's formulæ, and the theoretical intensity of the light reflected at 45° determined.

	Speculum Metal.		Steel.
Observed......	58·03	....	44·42
Calculated.....	58·56	....	48·98

The differences between the observed and calculated values are nearly the same as when the measurements were made in air.

Note by the Communicator. Received February 13, 1883.

The differences between the results of theory and observation as to the intensity of reflected light are in several of the above experiments so considerable, that we are led to ask whether there may not be something in the experiments to which it may be referred.

A slight inaccuracy in the calculated numbers is produced by the neglect of the polarisation due to oblique reflection from the paper employed. With glazed paper this might be considerable; but naturally a paper would be selected with as dull a surface as possible, and the author assures me that the polarisation was only just perceptible. It could be easily allowed for by measuring the amount of polarisation produced by the oblique reflection. When light of given intensity polarised first in and then perpendicularly to the plane of incidence is incident obliquely on the paper, let the intensities of the reflected

light be as r to 1. Then it will be easily seen that the theoretical intensity for common light reflected first from the metal, and then from the paper, as determined by the method of the paper, will be $(rJ^2+I^2)/(r+1)$ instead of $\frac{1}{2}(J^2+I^2)$. The former exceeds the latter by—

$$\frac{(r-1)(J^2-I^2)}{2(r+1)},$$

which is positive, since r is a little greater than 1, and J^2 is greater than I^2. This excess is very small, but as far as it goes it tends rather to increase, on the whole, the difference between theory and observation, since the observed intensity nearly always falls short of the calculated.

The imperfection of the polish in the case of such soft metals as silver and tin has, doubtless, much to do with it. But the polish of the steel seems to have been practically perfect, and yet this metal showed discrepancies, though not so great.

But I think there are strong reasons for believing that the ordinarily received formulæ for metals can only give a more or less approximate result.

MacCullagh was the first to show that by substituting for the refractive index in Fresnel's formulæ a complex imaginary, and then interpreting the formulæ as Fresnel has done in a somewhat analogous case, results were obtained agreeing, at any rate approximately, with those deduced from observation. Cauchy afterwards gave formulæ substantially the same, as they differ only in algebraic development, but made an important advance in the physical theory by connecting the coefficient of $\sqrt{-1}$ in the complex imaginary with an intense absorbing action of the medium.

But metals are not the only bodies to which the formulæ of Fresnel do not apply. More than fifty years ago Sir George Airy showed that in the case of diamond a considerable quantity of light polarised perpendicularly to the plane of incidence was reflected at the angle which made the nearest approach to a polarising angle; and that on increasing the angle of incidence through the angle of maximum polarisation there was a rapid retardation of phase. Similar phenomena were afterwards observed in other transparent substances of high refractive index, and more recently M. Jamin has observed them in transparent substances in general with a few exceptions.

The effect increases on the whole with the refractive index of the substance, but not in such a manner as to allow us to suppose that it is a function of the refractive index. Hence two independent constants are required to define for a given kind of homogeneous light the optical character of a transparent substance.

The *adamantine property* of a substance, as for the sake of a name it may be called, increasing on the whole with the index, and consequently on the whole with the density, there can be little doubt that if metals, retaining their actual density, were transparent like diamond, they would exhibit this property in a greatly exalted degree. On the other hand, the metallic properties connected with intense absorption are exhibited by many non-metallic substances, such for example as the colouring matters derived from aniline. Hence we have very strong reason for believing that there are *two distinct and independent* properties in a metal by virtue of either of which, if it stood alone, there would be a change of phase and persistence of intensity for light polarised perpendicularly to the plane of incidence as the angle of incidence was increased. The changes due to these two would not follow the same laws as regards their dependence on the angle of incidence. The coefficients expressing these two properties, together with what answers to the index of a transparent substance, make *three* physical constants which are required to define a metal optically, even in relation to homogeneous light.

If we confine our attention to an experimental determination of the difference of phase and ratio of intensity for light polarised in and perpendicularly to the plane of incidence, and if, neglecting altogether the adamantine property, we determine the two constants in the ordinary formulæ for metallic reflection so as to make them agree with observation in two cases, suppose by giving a difference of phase of 90° for an observed angle, and an observed ratio of intensities at that angle, it may be readily imagined that the ordinary formulæ may be to a certain extent formulæ of interpolation, giving the difference of phases and ratio of intensities for other angles of incidence without any very material error; and yet when we come to a totally different kind of observation, such as that of determining the ratio of the intensity of incident to that of reflected light, that the formulæ may be found to be very distinctly in error. Hence observations of this latter class seem deserving of more attention than have lately been bestowed upon them, lest from too great reliance on the accordance between theory and observation as regards the difference of phase and ratio of intensities when we compare light polarised in and perpendicularly to the plane of incidence, we should be led unduly to trust the formulæ, for giving correctly the ratio of intensities for incident and reflected light.—G.G.S.

II. "Description of an Apparatus employed at the Kew Observatory, Richmond, for the Examination of the Dark Glasses and Mirrors of Sextants." By G. M. WHIPPLE, B.Sc., Superintendent. Communicated by WARREN DE LA RUE, Esq., Vice-Chairman of the Kew Committee. Received February 6, 1883.

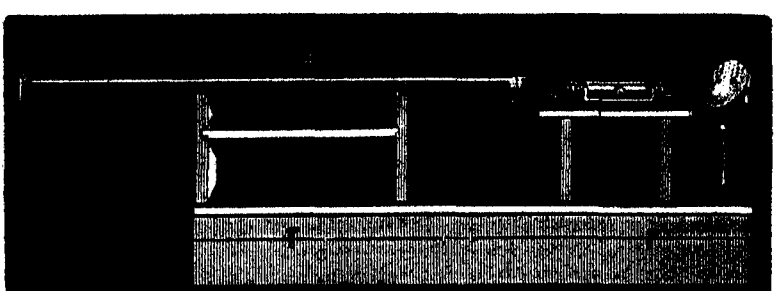
In the "Proc. Roy. Soc.," vol. 16, p, 2, Professor Balfour Stewart described an apparatus designed and constructed by Mr. T. Cooke for the determination of the errors of graduation of sextants. This instrument has from that date been constantly in use at the Kew Observatory, and since the introduction of certain unimportant improvements, has been found to work very well.

No provision was made, however, for its employment in the determination of the errors of the dark shades used to screen the observer's eyes when the sextant is directed to the sun or moon, and it has been found that errors may exist in the shape of want of parallelism in these glasses, sufficiently large to seriously affect an observation, accurate in other respects.

It has also been found that sextant makers are desirous of having the shades examined before proceeding to fit them into their metal mountings, and also to have the surfaces of the mirrors tested for distortion before making the instruments up. With a view to the accomplishment of these ends, for some time past the Kew Committee have undertaken to examine both dark glasses and mirrors, and to mark them with a hall-mark, when they are found to answer the requirements necessary for exactitude.

For these purposes the apparatus now described has been devised by the author, and brought into use at the Observatory.

It is represented in the annexed cut.



A telescope, A, of 3½ inches aperture and 48 inches focal length, a pair of collimators, B and C, of 1½ inch aperture and 10 inches focal length, and a heliostat, D, are firmly fixed to a stout plank, so

that their axes may be in the same horizontal plane. The eye-piece of the telescope, E, carries a parallel wire micrometer, F. G is the dark glass to be examined, and H is another glass of the same tint.

In order to adjust the instrument, the telescope, A, is directed to the sun, a shade being fitted to the eye-piece, and then placed in its Y's focussed for parallel rays. The collimators, B and C, are then fixed on their table with their object-glasses opposed to that of the telescope, A, the eye-pieces and wires having first been removed, and a metal plate with a sharply cut hole in its centre, fitted to their diaphragms.

Light is next reflected down the collimator by the mirror D, and the aperture in the diaphragm, being viewed through the telescope A, is carefully focussed by moving the object-glass of the collimator to and fro, by means of its rack and pinion.

The diaphragm aperture is next collimated by rotating the collimator in its bearings.

Both collimators being thus adjusted they are placed side by side, so that their illuminated sights can be viewed simultaneously in the telescope, appearing as superimposed bright disks 12′ in diameter. They are next separated so that the disks remain merely in contact at the extremity of their horizontal diameters.

The instrument is now ready for use, and the examination of the shades is performed in the following manner.

The glass to be tested is fixed in a holder, in front of the object-glass of collimator B, a corresponding shade being placed between the heliostat and diaphragm of collimator C. The sun is directed on to the diaphragms. The coloured disks are viewed through the telescope, when if the sides of the shade G are perfectly parallel the relative position of the disks is unchanged, if, however, the shade is not ground true, the disks will appear either separated or to overlap. In the first case, the amount of separation is measured by the micrometer, F, and serves to indicate the quality of the glass. In the case of overlapping images the shade is rotated through 180°, and separation produced which can be measured. A second examination is then made, the shade having been turned through 90°.

If in no position a separation of images is found to exist to the extent of 20″, the glass is etched K.O. 1; if more than 20″ but less than 40″, the mark is K.O. 2, with greater distortion than this, the shade is rejected and not marked.

To examine the quality of the mirrors, a small table, on levelling screws, is put in front of the object-glass of the telescope. The mirror to be tested is placed on its edge on this table, and turned until a distant well-defined object is reflected down the tube of the telescope. The object-glass of the telescope having previously been

stopped down to an aperture corresponding to the size of the mirror, the reflected image is contrasted with that seen directly, and if the definition is unchanged, the mirror is marked K.O., with a writing diamond, and returned to the maker; if the object appears distorted, its unfitness for use is similarly notified. A small fee is charged for the examination.

III. "On the Atomic Weight of Manganese." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor, Cambridge, and ALEXANDER SCOTT, M.A. Received February 9, 1883.

Our attention has been directed for some time to a new determination of the atomic weight of manganese. This communication gives a succinct account of the results of the preliminary stages of such an inquiry, and although the further progress of the investigation may reveal some errors, still we feel convinced the final numbers can in no way differ materially from the present values, and therefore further delay in publication is unnecessary.

The atomic weight of manganese has been determined by many chemists,* but the resulting values vary considerably according to the special method selected. The results of the different investigators may be divided into two classes—those giving approximately 55 as the number, and those making it about 54. To the former class belong Turner, Berzelius, and Dumas, all of whom use the same method, viz., the determination of the silver chloride yielded by a weighed amount of chloride of manganese. Turner also made determinations from the analysis of the carbonate, and from the conversion of the monoxide into sulphate. Von Hauer used the same method as that employed by him in the determination of the atomic weight of cadmium, viz., the reduction of manganous sulphate to sulphide by ignition in a current of sulphuretted hydrogen. It is probable that this method is not very trustworthy, as, according to Schneider, the sulphide may be contaminated by oxysulphide. Schneider and Rawack belong to the second class of observers, the former employing the oxalate, and from its analysis calculating the atomic weight by deducting the weight of water and carbon dioxide obtained. Rawack, whose experiments were conducted in Schneider's laboratory, weighed the water obtained by reducing manganoso-manganic oxide to manganous oxide.

One objection to the analysis of the chloride is that it may contain besides manganous chloride varying proportions of manganic salt.

* Berzelius, "Lehrbuch," 5 Ed., 3, 1224. Dumas, "Ann. Chem. Pharm.," 113, 25, 1860. Hauer, "Wien. Acad.," xxv, 124. Rawack and Schneider, "Pogg. Ann.," 107, 603.

This must be the case if, as Forchammer maintains, pure manganous salts are colourless, the pink colour of manganese salts being due to traces of a manganic compound. Forchammer has observed that on fusing manganese sulphate with potassium hydrogen sulphate, a white mass is obtained which gives a colourless solution. We have been unable to prepare any chloride or bromide without a pink or rose colour giving a correspondingly coloured solution, and this was also the case with specimens fused in hydrogen and hydrochloric acid gas. The effect of a trace of manganic salt in the chloride would be to lower the atomic weight. The chloride and bromide of manganese are both not only very hygroscopic, but if fused in hydrochloric acid gas (or hydrobromic acid in the case of the bromide) are liable to retain traces of the halogen acids, and this would consequently make the atomic weight too low.

In order to ascertain the values of the atomic weight of manganese which result from careful analysis of the halogen salts, determinations were made of the molecular weights of chloride and bromide on specimens prepared with great care. The number found for the bromide was 214·87, and for the chloride 125·825, yielding the respective atomic weights of manganese of 54·97 and 54·91. All researches on the oxides of manganese have shown that they are all difficult to obtain in anything like a definite form, with perhaps the exception of the protoxide.

It occurred to us that the analysis of silver permanganate might be employed with advantage, as this salt is found in a very definite state, and can be easily freed from all the allied metals. The selection of this substance, moreover, involved only the atomic weights of silver and oxygen, and as it seemed feasible to deduce the atomic weight of the manganese directly from the percentage loss of oxygen on heating, we expected to get very accurate results. In this we were disappointed, as we have not been able to obtain concordant results by this most direct method.

Table I.

	Weight of silver permanganate.		Weight of residue. Ag + MnO.		Oxygen lost.	Equivalent.
	In air.	In vacuo.	In air.	In vacuo.		
I....	5·8688	5·8696	4·6320	4·63212	1·23748	227·673
II ..	5·4981	5·4988	4·3358	4·33591	1·16293	226·965
III..	7·6725	7·6735	6·0538	6·05395	1·61959	227·422
IV ..	13·0997	13·10147	10·3179	10·31815	2·78332	225·943
V...	12·5782	12·5799	{ 9·9104	9·91065	2·66925	226·22
			9·9141	9·91435	2·66555	226·53

Table I gives the results of the direct determination of the equivalent of the permanganate of silver by reduction in hydrogen.

The silver permanganate was heated in a bulb of hard glass, first in a current of pure air and then in hydrogen, at a red heat, until the resulting mixture of silver and oxide of manganese had a constant weight. The residue was allowed to cool in hydrogen, which was finally displaced by nitrogen before weighing. The results obtained by this method show great variation, the errors being probably due to the occlusion of hydrogen and the suspension of some oxide of manganese in the oxygen evolved. The method finally employed was to dissolve the permanganate of silver in dilute nitric acid in presence of various reducing agents, such as sulphurous acid, sodium formate, and potassium nitrite. The silver was then determined by adding very nearly an equivalent quantity of pure potassium bromide, and titrating the small amount of silver remaining in solution, by means of very dilute potassium bromide, containing about 1·19 mgrms. of the pure salt per gramme of solution. The solutions were in all cases weighed, thus avoiding errors due to fluid expansion, faulty graduation of burettes, &c. The titrations were performed in yellow light in an apparatus similar to that used by Stas, and with all the precautions insisted on by him as essential to the accuracy of such determinations.

The permanganate of silver crystallises readily from warm water, and is a very stable salt. It is also quite anhydrous and not in the slightest degree hygroscopic. From its small solubility it is easily freed from adhering impurities by recrystallisation. The purity of the salt was tested by reducing about 5 grms. by means of alcohol and filtering, when the total residue only weighed 1·9 mgrms. This residue when tested with the spectroscope was found to consist almost entirely of calcium salts from an accidental impurity in the distilled water, only the faintest trace of potassium being detected. The sample which was tested in this way had only been recrystallised once after precipitation. The salt was usually prepared by the precipitation of silver nitrate by means of an equivalent quantity of potassium permanganate, the solutions being warm, and the silver permanganate thus obtained in fine needles, was easily drained, washed, and recrystallised. A quantity of the salt was also prepared from crystallised barium permanganate, which was made from barium chloride and silver permanganate, the barium salt being afterwards decomposed with pure silver sulphate. This method of preparing the permanganate of silver ensures the absence of any trace of silver nitrate, which as Stas has shown adheres most persistently to many silver salts.

Permanganate of silver has several very important advantages over the other bodies previously used for the determination of the atomic weight of manganese. Its freedom from hygroscopic properties and

the improbability of its containing excess of any of the elements of which it is composed beyond what is necessary for the formation of the normal compound, recommend it especially for this purpose. Another point which rendered its selection important was to ascertain if a body liable to partial decomposition under certain circumstances could give concordant results in atomic weight determinations, thus putting to a crucial test the amount of variation in the values which may be attributed to secondary causes.

Table II gives the results of the titrations. The use of sulphurous acid as the reducing agent was found unsatisfactory, as a slight residue having the appearance of sulphide was almost always left undissolved. The production of sulphate was also more or less troublesome from its insolubility.

Table II.

No.	$AgMnO_4$.	$AgMnO_4$. Corrected for vacuo.	KBr.	KBr. Corrected for vacuo.	Equivalent of $AgMnO_4$.	Reducing agent.
1	6·528	6·5289	3·4228	3·42385	227·091	Sulphurous Acid.
2	7·5368	7·5378	3·9541	3·9553	226·958	Nitrite of Potash.
3	6·1000	6·1008	3·20067	3·20166	226·937	" "
4	5·7457	5·74647	3·00584	3·00677	227·606	Sulphurous Acid.
5	6·1651	6·16593	3·23503	3·23602	226·918	Formate of Soda.
6	5·1126	5·11329	2·68216	2·6828	226·984	" "
7	5·0737	5·07438	2·6614	2·66204	227·013	Nitrite of Potash.
8	13·4466	13·4484	7·05385	7·05602	226·983	" "
9	12·5782	12·5799	6·59861	6·60065	226·972	Hydrogen.
10	12·2686	12·27025	6·4361	6·43808	226·976	Nitrite of Potash.

Experiments (6) and (7) were made with a sample obtained from the barium salt, and these results are slightly higher owing to the presence of a small trace of barium sulphate easily recognisable by the slight turbidity of the reduced solution. We had hoped by the use of a larger quantity of material to arrive at results comparable in some degree at least with those of Stas; but we found the preparation of considerable quantities of material of absolute purity frequently involves sources of error not incurred in the production of smaller quantities. This we observed especially in the preparation of our pure potassium bromide, which contained traces of sulphates in every sample. This sulphate is due to the use of ordinary gas in the ignition of the pure bitartrate of potash from which the bromide of potassium was made. In order to get a pure product gas must be replaced by a powerful flame of alcohol, or all the operations conducted in a muffle.

The mean atomic weight of manganese which results from the

average of the eight determinations in which sulphurous acid was not employed as the reducing agent is 55·038, oxygen being taken as 16 and silver as Stas's value 107·93.

Thus another element is added to the list of those whose atomic weights have been found on revision to be exceedingly near whole numbers.

Further details and discussion must be reserved for another communication.

February 22, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Effects of Temperature on the Electromotive Force and Resistance of Batteries." By WILLIAM HENRY PREECE, F.R.S., Received February 8, 1883.

It is well known that heat influences the conditions of galvanic elements so as to vary the strength of the currents generated by them in those parts of the circuits connecting their poles.

In 1840 De la Rive* found that the action of a galvanic pair was accelerated when it was put into hot fluid instead of cold fluid, and he attributed the result to increased chemical affinity.

Faraday† repeated De la Rive's experiment, but he, on the other hand, attributed the result to improved conductivity in the liquid, and he showed that the effect was not due either to motion, to chemical action, or to thermo-electric action, or indeed to any increase in the electromotive force.

Daniell,‡ also, found an increased current due to increased temperature. According to him, one of his elements was nearly trebled in strength when raised to 212° F. He attributed the effect to the increased energy of the affinity. Said he, "Changes of temperature even have a marked influence upon the working of the voltaic battery, and must not be neglected in nice comparative experiments."

J. B. Cooke§ made careful observations on the chemical affinity in a

* "Ann. Chem.," 1828, xxxvii, p. 242.
† "Researches," 17th Series, §§ 1925–26.
‡ "Chemical Philosophy," p. 506.
§ "Phil. Mag.," 1861, p. 95.

galvanic cell, and indicated the error due to changes of temperature, but he remarked, "These affinities do not appear to be affected by any changes of temperature between ranges of 50° and 212° F."

Crova investigated the effects of heat on the electromotive force alone, and he showed (1) that the electromotive force of a Daniell's element decreases regularly with an increase of temperature; (2) that the electromotive force of a Grove's element increases with temperature; (3) that the electromotive force of a single-fluid element of the Smee type is independent of the variation of temperature.

In 1862 Mr. James Dixon took advantage of the influence of heat to take out a patent for hot batteries, and he suggested the employment of these batteries for the production of the electric light.

In the same year (1862) Lindig* indicated a variation with changes of temperature.

In 1870 Bleekrode† made some further experiments in the same direction.

In 1872 Mr. Latimer Clark‡ showed that the electromotive force of his standard cell varied inversely with temperature about ·06 per cent. for each degree Centigrade.

In 1881 Herwig§ investigated the subject carefully, and showed that polarisation diminishes with temperature. He found that resistance decreased markedly with temperature, and that this was more evident with small electromotive forces than with powerful electromotive forces.

In 1878 the author|| in investigating the peculiar action of Byrne's pneumatic battery, showed that its exceptional power was due to an abnormal formation of heat in its interior, and that this acted, principally, in reducing the internal resistance.

As bearing indirectly also upon this question it should be noted that Becquerel, Paalzow, and Kohlrausch and Nippoldt examined the influence of heat upon the resistance of electrolytes, and showed that it invariably diminished as the temperature rose. This was determined by them for various solutions.

Now it is to be observed that in all these enquiries no one has quantitatively separated the influence of temperature upon electromotive force from its influence upon internal resistance. It is quite evident, from an examination of Ohm's law, that the variation in the strength of current can be the result either of a variation in the electromotive force alone or in a variation of the resistance alone, or in an unequal variation of both together. The numerous discre-

* "Phil. Mag.," 1865, I, p. 408.

† "Phil. Mag.," 1870, p. 310.

‡ "Phil. Trans.," 1873.

§ "Ann. Phys.," B. XI, H. 4, No. 12, p. 661.

|| "Proc. Soc. Telegraph Engineers," 1878.

pancies that have appeared in the measurements of the behaviour of the Daniell's cell, as well as the erratic performance of batteries used for telegraphic purposes in various exposed positions, have long attracted the attention of the author to the necessity of a more careful enquiry into this matter than has been made hitherto. In fact, all the observations that have hitherto been made are positively useless, from the simple fact that no record has been kept of the independent variations of the internal resistance and the electromotive force in any measurable or comparable manner.

Special apparatus was made, and the following method of experimenting was decided upon.

The cell to be experimented upon was placed inside a cylindrical copper vessel about 10 inches high and about 8 inches diameter; water was poured into the vessel to within an inch of the top of the cell, and the lid of the vessel was put on.

This lid had four holes, two (insulated from the rest of the vessel) to receive the electrodes of the cell, and the other two to allow thermometers to be plunged into the liquid or liquids in the cell without removing the cover or lid. The water in the vessel and the cell in the water were then heated by means of a gas-burner placed underneath the vessel, and the electromotive force and the resistance of the cell were determined at various stages of the heating; while the temperature of the cell was observed at the time of each experiment, the liquid or liquids in the cell were stirred up from time to time so as to obtain, as far as possible, the true temperature of the cell.

The cells experimented upon were the Daniell, bichromate, and Leclanché; those in general use, especially for telegraph purposes.

The Daniell cell consists of a porous pot, containing a solution of copper sulphate, and placed in a stoneware vessel, containing a solution of zinc sulphate. In the porous pot is immersed a copper plate, bent so as to form a hollow cylinder, to which is soldered a copper wire, which constitutes one pole of the cell. A zinc plate, also bent into a cylindrical form, is placed in the outer cell, and has a copper wire soldered to it, constituting the other pole of the cell. The zinc is not amalgamated.

Two forms of the bichromate cell were experimented on, the one being that known as Fuller's bichromate cell, and the other that known as single-fluid bichromate cell. The double-fluid cell (Fuller's) consists of a stoneware jar of a quart size. Inside this is placed a porous pot, in which the zinc is placed; the negative plate, which is of carbon, is placed in the outer jar; the zinc is cast in the form of a short truncated cone. It is cast on a stout copper wire, both are well amalgamated; and the plate is surmounted by a terminal. In the outer jar is placed 3 ozs. of bichromate of potassium and 4 ozs. of sulphuric acid. In the inner pot is placed 2 ozs. of mercury. Both

are then filled up to within 2 inches of the top with a weak acid solution (one part of sulphuric acid to nine parts of water).

The single-fluid bichromate cell is no more than the cell just described, in which are placed nothing but a rod of amalgamated zinc and a plate of carbon, each forming a pole of the cell.

The Leclanché cell is made thus:—Into a glass jar a solution of the ordinary commercial sal-ammoniac is poured. A zinc rod or plate into which a connecting tinned iron wire has been cast, is then placed in the solution, and a plate of carbon surrounded by a mixture of broken gas-carbon or coke and peroxide of manganese, is fixed in a small porous pot at the top of the jar. To make an attachment for the terminal, the top of the carbon plate is capped with lead, which makes good contact with the carbon and is not liable to be attacked by ammonia. The carbon plate is then dipped in melted paraffin, to fill up its pores and to check the ascension of the liquid by capillary action. Lastly, the wire, the top of the zinc rod, and the lead cap of the carbon plate are covered with pitch, ozokerit, marine glue, or some other compound to protect them from local action.

The results of the experiments are given in the tables below, in which the electromotive force (e) is given in terms of that of a Daniell cell (a standard cell) in good order and at about 14° C., and the resistance is given in B.A. units. The experiments were conducted for me by Mr. R. Shida with great care and patience. Of the Tables I, II, and III, which contain the results of the experiments on the Daniell cell, the first two tables refer to the case where the solution of copper sulphate was saturated at all temperatures (that is to say, the crystals of copper sulphate were always present in the solution) and the solution of zinc sulphate was kept the same, or nearly the same, in strength (that is, the solution was saturated at about 14° C.); Table III refers to the case where both copper sulphate and zinc sulphate solutions were kept unaltered or nearly so in strength during the experiment (that is to say, they were both saturated at about 10° C.). Table IV contains the results for the double-fluid bichromate; Table V those for the single-fluid bichromate; and Table VI those for the Leclanché.

(I.) The Electromotive Force.

(*a.*) *The Daniell Cell.*—Tables I, II, and III show that, as the cell was heated up from a comparatively low temperature to a higher and higher temperature, the electromotive force of the Daniell cell decreased rather abruptly at first, but more gradually afterwards, until, at a certain temperature it began to increase and continued to do so till the temperature attained that of the boiling point of water; but that (which is rather singular) the electromotive force remained unaltered, or nearly so, while the cell was being cooled down from 100° C. to a lower and lower temperature. These peculiar results (together with

the fact that whereas the temperature of the zinc cell rose faster than that of the copper cell while being heated up, the former cooled faster than the latter while cooled down from a high temperature [say 100° C.]), tend to indicate that the diminution in the electromotive force of the cell at the beginning of the experiment was greatly, if not chiefly, due to the thermo-electric action which must have been set up in the circuit.

(*b.*) *The Bichromate Cell.*—It will be seen from the Tables IV and V that the electromotive force of the bichromate (both the double-fluid and the single-fluid cell), diminished regularly when the temperature was made higher and higher, and increased regularly when the temperature was made lower and lower. The regular diminution by rise and regular augmentation by fall of temperature of the electromotive force (at least when the range of temperature was between 0° C. and 100° C.), was very much greater in the case of the single-fluid bichromate than in the case of the double-fluid bichromate. In the case of the first, the electromotive force at 100° C. was as much as about 6 per cent. lower than that at about 14° C.; whereas in the case of the second, the electromotive force at 100° C. was only about 1·6 per cent. lower than at 19° C.

(*c.*) *The Leclanché Cell.*—The Table VI shows that the electromotive force of the Leclanché varied, when the temperature was varied, so slightly, if at all, that it was difficult to observe the variation by the method used.

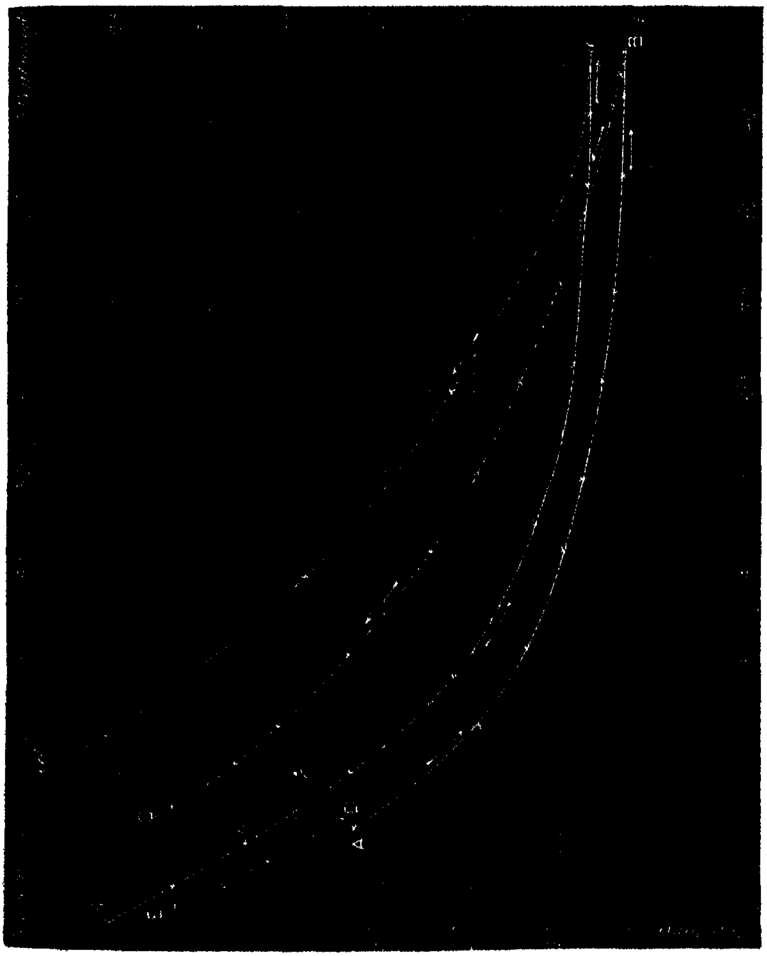
(II.) The Internal Resistance.

The results obtained for the resistances of the various kinds of cells are more striking and more interesting than those for the electromotive force, as will be quite evident from the tables. They will, however, be understood more readily from the graphical representations of the results by means of curves (in the Diagrams I, II, and III) in which the abscissæ are proportional to the resistances in B.A. units, and the ordinates to the temperatures in degrees Centigrade.

(*a.*) *The Daniell Cell.*—The curves in the Diagram I represent the results for the Daniell, the curve ABCDE corresponding to the case (Table II) in which the copper sulphate solution was kept saturated at all temperatures, while the zinc sulphate solution was kept constant in strength, and the curve *abcde* corresponding to the case (Table III) in which both solutions were kept unaltered in strength during the experiments. The directions of the arrows indicate the order of the experiments. For instance, in the curve ABCDE the portion AB represents the result obtained while the cell was being heated up from about 11° C. to nearly the boiling point of water, the portion BC that obtained while the cell thus heated up was being cooled down from 100° C. to about 12° C.; and, lastly, the portion DE that

obtained in cooling the cell from 12° C. to nearly so low a temperature as the freezing point of water. A very similar explanation applies to the curve *abcde.*

DIAGRAM I.



The results thus laid down by means of the curves ABCDE and *abcde* present many points of interest. These curves clearly show:—

1°. That when a Daniell cell is heated from a low temperature, say 0° C., up to a high temperature, say 100° C., the resistance of the cell decreases rather abruptly at first, but more gradually afterwards, falling from 2·12 to 0·66 ohm, or more than one-third.

2°. That when the cell thus heated up is cooled down, the resistance

increases, but increases at a greater rate than it decreased while being heated; *in other words, the resistance of a* Daniell cell *at any temperature* (at least between 0° C. and 100° C.) is smaller before it is heated up to a high temperature than afterwards, provided the heating and cooling be done not very slowly.

3°. That if the cell thus cooled down be left undisturbed at a certain temperature, the resistance of the cell gets less and less, till, at last, at the end of a certain period (which will be from about 40 to 50 hours), it gets down to the value which it had before being heated up at all.

4°. And, lastly, that the resistance of a Daniell cell is considerably less when the solution of the copper sulphate is more concentrated than when it is less concentrated, at any temperature, and under otherwise exactly similar circumstances.

(*b*.) *The Bichromate Cell.*—The results for the bichromate are not quite so remarkable, nor are they so interesting as those for the Daniell cell, as will be seen on comparing the curves in the Diagram I with those in the Diagram II, but the fall of resistance is nevertheless very striking. In the case of the double-fluid bichromate the curve HK of resistance obtained while the cell was being heated up, differs so slightly from the curve KL obtained while it was being cooled down, that the one is hardly distinguishable from the other. The differences in fact may be attributed more to errors of observation than to anything else. Yet, if there should be any difference in the resistance in the two cases, it is one opposite in character to that found in the case of the Daniell cell.

The probability of the existence of this difference, as indicated by the curve HKL, is supported by the results shown by means of the curve *hlk*, for the single-fluid bichromate cell. Every point of the portion *hk* of the curve obtained while heating the cell up, lies considerably higher than the corresponding point in the portion *kl* obtained while cooling it down; that is to say, the resistance of the single-fluid bichromate cell at any temperature is greater before than after it has been heated up.

(*c*.) *The Leclanché Cell.*—Very little remains to be said of the curve PQR in the Diagram III, which represents the results for the Leclanché cell. The general character of the curve PQR bears a strong resemblance to the curve HKL; in other words, the resistance of the Leclanché diminishes with the rise and increases with the fall of temperature at nearly the same rate as the resistance of the double-fluid bichromate cell does. And, moreover, it is a matter of difficulty to say with certainty, in the case of the Leclanché as in the case of the double-fluid bichromate, whether or not the curve of resistance obtained while being heated up coincides with the curve of resistance obtained while being cooled down, because the part RQ is so nearly coincident with the part PQ of the curve PQR, that any slight errors

in the observations may have caused the non-coincidence between the two parts.

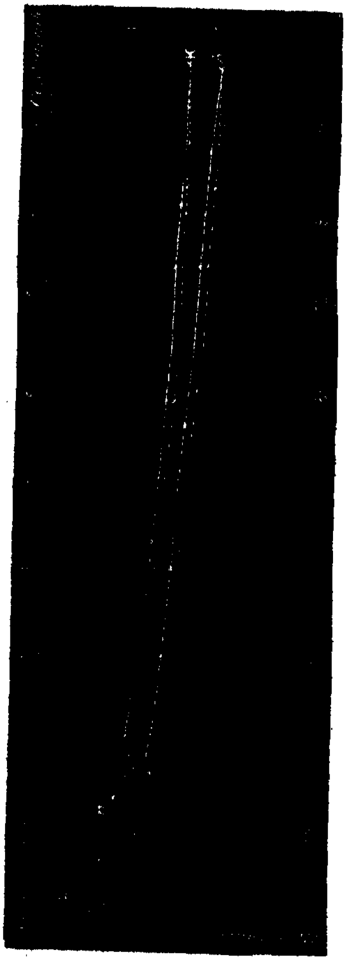


Diagram II.

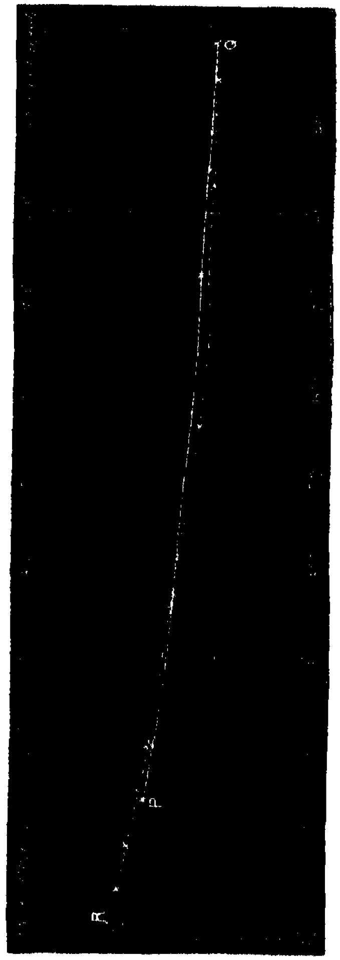


Diagram III.

It follows from these experiments that changes of temperature do not practically affect electromotive forces, but that they materially affect the internal resistances, of cells. Faraday's observation is fully confirmed, while Daniell's mistake is easily understood if he employed, as he probably did, a galvanometer of low resistance.

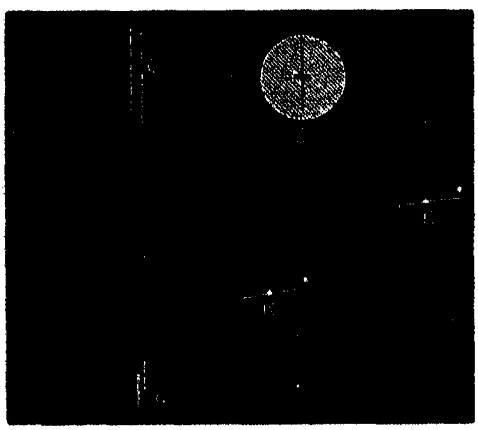
It also follows that of the various forms of batteries in practical use the Daniell is most seriously influenced by variations in temperature, and that in all experiments with that battery, either the temperature must be kept constant, or frequent measurements should be taken of its internal resistance and allowance made for the variation.

Received February 20, 1883.

Note.—The method adopted to measure the electromotive force and the resistance of the different cells was very simple, and, as it is believed to be very accurate and free from any of the disturbing influences due to polarisation, a description of it may be useful.

The charge or discharge of a small condenser through a galvanometer of comparatively low resistance is an accurate measure of the electromotive force present, for the current is practically instantaneous, and therefore the whole quantity present acts as a balista upon the needle.

In the figure C is a condenser of ·3-microfarad capacity, G a sensitive reflecting galvanometer, b the cell to be measured, r a shunt of small resistance, and K_1 and K_2 simple keys.



The condenser is first charged with a standard Daniell cell and the charge deflection (D) noted. The standard cell is then replaced by the cell b to be examined, and the charge deflection (d) produced by it noted. The key K_2 is then depressed (K_1, which had been previously depressed to charge the condenser, being still held down), and the cell is thus shunted through r. The electromotive force affecting the condenser is thus reduced and a discharge (d') is noted—the deflection being in the reverse direction.

Now if b be the resistance of the cell under test, then

$$b = r\frac{d'}{d-d'}.$$

This particular mode of measuring was devised by Mr. H. R. Kempe and modified by Mr. Munro. Condensers are much used in telegraphy for measuring electromotive forces. [*Vide* Kempe's "Handbook of Testing," p. 195.]

Table I.

Daniell cell.—Zinc sulphate solution saturated at about 14° C., and copper sulphate solution saturated at all temperatures.

Time.		Temperature.			D.	d.	d'.	r.	e.	b.	
Date.	Hour.	Copper sulphate solution.	Mean.	Zinc sulphate solution.							
Dec. 9	11 A.M.	13° C.	13° C.	13° C.	288	288	119	3	1·000	2·12	Heating commenced.
		26	23	20	,,	285	94	,,	·990	1·48	
		40	33	26	,,	285	81	,,	·990	1·19	
		52	43·5	35	,,	285	70	,,	·990	·98	
		64	56·5	49	,,	285·5	61	,,	·991	·82	
		78	71·5	65	,,	286·5	56	,,	·935	·73	
		84	79·5	75	,,	286·5	54	,,	·995	·70	
		86	83	80	,,	286·5	53	,,	·995	·68	
		89	86	83	,,	286·5	53	,,	·995	·68	
		98	95·5	93	,,	287	52	,,	·996	·66	
		100	100	100	,,	287	52	,,	·997	·66	Heating stopped.
		95	95·5	96	,,	288	58	,,	1·000	·76	
		90	90·5	91	,,	288	62	,,	1·000	·82	
		80	81·5	83	,,	288	68	,,	1·000	·93	
		70	70·5	71	,,	288	76	,,	1·000	1·08	Hot water removed and cold water poured in the vessel.
		60	60	60	,,	286	88	,,	1·000	1·32	
		47	47	47	,,	288	104	,,	1·000	1·70	
		35	35	35	,,	288	122	,,	1·000	2·20	
Dec. 9	7 P.M.	25	25	25	,,	288	134	,,	1·000	2·61	Experiments stopped and everything left undisturbed till the morning of the 11th.
Dec. 11	10.30 A.M.	11	11	11	288	288	122	,,	1·000	2·18	

N.B.—If E and e be the electromotive force of the standard cell and the cell examined respectively, and b be the resistance of the latter cell,

Then $$e = E \times \frac{d}{D}, \qquad b = r \times \frac{d'}{d - d'}.$$

Table II.

Daniell Cell.—*Zinc sulphate solution saturated at 14° C., and copper sulphate solution kept saturated at all temperatures.*

Time.		Temperature.			D.	d.	d'.	r.	e.	b.	Remarks.
Date.	Hour.	Copper sulphate solution.	Mean.	Zinc sulphate solution.							
Dec. 11	10.30 A.M.	11° C.	11° C.	11° C.	288	288	122	3	1·000	2·18	Heating commenced.
		25	22	19	,,	285	94	,,	·989	1·48	
		36	31	26	,,	284·5	82	,,	·988	1·21	
		48	42	36	,,	285	72	,,	·989	1·01	
		56	50	44	,,	285·5	66	,,	·991	·92	
		68	61·5	55	,,	286	59	,,	·993	·78	
		78	71·5	65	,,	287	54	,,	·996	·70	
		88	84	80	,,	287	53	,,	·996	·68	
		96	93·5	91	,,	288	52	,,	1·000	·66	
		99	98	97	,,	288	52	,,	1·000	·66	Heating stopped, lid of vessel taken off, hot water removed and cold water poured in.
		95	95·5	96	289	289	54	,,	1·000	·68	
		91	92	93	,,	289	58	,,	1·000	·75	
		82	83	84	,,	289	66	,,	1·000	·89	
		71	72	73	,,	289	73	,,	1·000	1·01	
		59	61	63	,,	289	84	,,	1·000	1·23	
		50	50·5	51	,,	289	95	,,	1·000	1·47	
		41·5	42	42·5	,,	289	106	,,	1·000	1·74	
		30	30·5	31	,,	289	124	,,	1·000	2·25	
		21	22	23	,,	289	134	,,	1·000	2·60	
		17	18	19	,,	289	141	,,	1·000	2·86	
Dec. 11	7.15 P.M.	13	13	13	,,	289	149	,,	1·000	3·19	Experiment stopped and everything left undisturbed till next morning.
Dec. 12	11.40 A.M.	12	12	12	,,	289	123	,,	1·000	2·22	Cooling by means of ice commenced.
		6	7	8	,,	289	136	,,	1·000	2·66	
		3	4	5	,,	288	142	,,	·996	2·90	
,,	1 P.M.	2	2	2	,,	288	146	,,	·996	3·06	

Table III.

Daniell Cell.—Both zinc sulphate and copper sulphate solution very nearly saturated at about 10°C.

Time.		Temperature.			D.	*d*.	*d'*.	*r*.	*e*.	*b*.	Remarks.
Date.	Hour.	Copper sulphate solution.	Mean.	Zinc sulphate solution.							
Dec. 19.	1 P.M.	7° C.	9° C.	11° C.	227	227	108	3	1·000	2·72	Cooling by ice and salt commenced.
		3	4	5	„	227	117	„	1·000	3·19	
	3. 15 P.M.	0	0	0	„	227	123	„	1·000	3·55	Experiment stopped.
					232	232	98	„	1·000	2·19	
Dec. 20.	10. 20 A.M.	17	17	17	232	232	98	„	1·000	2·19	Commenced heating.
		34	28	22	„	230	80	„	·991	1·60	
		52	45	38	„	229	63	„	·987	1·14	
		70	63	56	„	229	54	„	·987	·93	
		84	79	74	„	230	53	„	·991	·90	
		93	91	89	„	230	52	„	·991	·88	
	2. 0 P.M.	99	98	97	„	230	52	„	·991	·88	Stopped heating.
		86	84	82	„	231	56	„	·996	·96	
		55	60	65	„	232	82	„	1·000	1·64	
		35	39	43	„	332	104	„	1·000	2·44	
		20	21	22	„	232	126	„	1·000	3·57	
	6. 40 P.M.	17	17	17	„	232	130	„	1·000	3·87	Experiments stopped and everything left undisturbed till next day.
Dec. 21.	11 A.M.	17	17	17	232	232	106	„	1·000	2·52	

Table IV.

Double-fluid Bichromate Cell.

Time.		Temperature.			$\dot{D}$.	d.	d'.	r.	e.	b.	Remarks.
Date.	Hour.	Carbon cell.	Mean.	Zinc cell.							
Dec. 14.	11.10 A.M.	14° C.	14° C.	14° C.	222	429	219	1	1·932	1·04	Heating commenced.
		23	25	27	,,	426	208	,,	1·919	·95	
		48	43	38	,,	425	195	,,	1·914	·85	
		62	59	56	,,	424	188	,,	1·910	·80	
		79	74	69	,,	423	180	,,	1·905	·74	
		84	81	78	,,	423	177	,,	1·905	·72	
		92	88	86	,,	423	172	,,	1·905	·69	
		98	97	96	,,	423	168	,,	1·905	·66	Heating stopped.
		84	83	82	,,	423	174	,,	1·905	·70	
		52	59	66	,,	425	182	,,	1·914	·75	
		41	45	47	,,	426	194	,,	1·919	·84	
		25	27	29	,,	428	208	,,	1·928	·95	Experiments stopped and everything left undisturbed till December 16th.
Dec. 14.	5.15 P.M.	17	17	17	,,	429	217	,,	1·932	1·02	
Dec. 16.	10.20 P.M.	14	14	14	,,	429	219	,,	1·932	1·04	
		6	7	8	,,	429	229	,,	1·932	1·14	Process of cooling by ice commenced.
		4	4·5	5	,,	429	234	,,	1·932	1·20	
Dec. 16.	2 P.M.	2·5	3	3·5	,,	429	237	,,	1·932	1·23	

Table V.

Single-fluid Bichromate Cell.

Time.		Temperature of the cell.	D.	*d.*	*d'.*	*r.*	*e.*	*b.*	Remarks.
Date.	Hour.								
Dec. 28.	10.50 A.M.	19° C.	212	408	190	1	1·924	·87	Heating commenced.
		29	,,	405	182	,,	1·910	·81	
		40	,,	404	177	,,	1·906	·75	
		56	,,	403	167	,,	1·901	·71	
		74	,,	398	156	,,	1·878	·66	
		87	,,	392	140	,,	1·849	·56	
		92	,,	386	131	,,	1·820	·51	
		96	,,	384	128	,,	1·810	·50	Heating stopped.
		83	,,	388	135	,,	1·830	·53	
		74	,,	392	140	,,	1·849	·56	
		60	,,	397	152	,,	1·872	·62	
		42	,,	404	169	,,	1·906	·72	
		17	,,	408	184	,,	1·924	·82	Cooling by ice commenced.
		10	,,	408	196	,,	1·924	·92	
		2	,,	408	222	,,	1·924	1·11	

Table VI.

Leclanché Cell.

Time.		Temperature.			D.	*d*.	*d'*.	*r*.	*e*.	*b*.	Remarks.
Date.	Hour.	Zinc cell.	Mean.	Carbon cell.							
Dec. 18.	10.45 A.M.	14° C.	14° C.	14° C.	227	312	146	1	1·375	·87	
		22	20	18	„	312	142	„	1·375	·84	Heating commenced.
		40	36	32	„	312	134	„	1·375	·75	
		60	57	54	„	312	120	„	1·375	·63	
		76	73	70	„	312	116	„	1·375	·59	
		87	84·5	82	„	312	112	„	1·375	·56	
		92	89	86	„	312	110	„	1·375	·54	
		94	91	88	„	312	108	„	1·375	·53	
		97	95	93	„	312	106	„	1·375	·51	Heating stopped.
		80	83	86	„	314	110	„	1·383	·54	
		52	56	60	„	313	124	„	1·379	·66	
		34	35	36	„	312	136	„	1·375	·77	
Dec. 18.	6.15 P.M.	20	21	22	„	312	145	„	1·375	·87	Stopped observations and everything left undisturbed till next day.
Dec. 19	11 A.M.	15	15	15	„	312	148	„	1·375	·90	
		8	9	10	„	313	154	„	1·379	·97	Process of cooling by ice and salt commenced.
		3	4	5	„	312	158	„	1·375	1·03	
		0	0	0	„	312	164	„	1·375	1·10	

II. "Preliminary Note on the Action of Calcium, Barium, and Potassium on Muscle." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received February 13, 1883.

It has been shown by Ringer that calcium prolongs the contraction of the frog's heart. This prolongation is diminished by the subsequent addition of potash.

It occurred to us that calcium and potassium salts might exercise a similar action on voluntary muscle. On trying it, we found this to be the case. Calcium in dilute solution prolongs the duration of the contraction in the gastrocnemius of the frog. Potassium salts subsequently applied shorten the contraction. We have been led to try the effect of barium on muscle by considerations regarding the relations of groups of elements, according to Mendelejeff's classification, to their physiological action. These considerations we purpose to develop in another paper. The effect of barium is very remarkable. It produces a curve very much like that caused by veratria, both in its form and in the modifications produced in it by repeated stimuli. We have found that the veratria curve is restored by potash to the normal in the case of the gastrocnemius, just as Ringer found it in the case of the frog's heart. The peculiarity which barium produces in the gastrocnemius is also abolished by potash. We have tested a number of other substances belonging to allied groups, and find that some of them have a similar, though not identical, action with barium. The results of these experiments, as well as the general considerations to which we have already alluded, we purpose to discuss in another paper.

III. "On the Formation of Uric Acid in the Animal Economy and its relation to Hippuric Acid." By ALFRED BARING GARROD, M.D., F.R.S. Received February 15, 1883.

(Abstract.)

The results which have been arrived at, and discussed in this communication, may be summed up as follows:—

Introduction.—The solubility of uric acid and of some of its more important salts at the temperature of the healthy human body has been determined and arranged in a tabular form. These figures may be useful for future reference.

The action of urates of ammonium and sodium upon chlorides and phosphates of the same bases, when mixed with each other in different proportions, has been ascertained.

Part I.—The results of many fresh observations, which have been made on the composition of the urinary excretion in several of the lower animals, are given.

The physical and microscopic characters of the semi-solid urines of birds and reptiles and invertebrata have been investigated at length, and it is shown that the urate is always in the form of spherule aggregates, made up of a great number of smaller spherules; that each of these is united with or contained in a cell of colloid matter, and that when treated with water and weak carbonated alkaline solutions, this solid urine swells up to very many times its original bulk.

The results of an investigation into the chemical composition of such urates are stated in a tabular form.

An examination of the blood of man and many mammals and birds and reptiles, especially in relation to uric acid, has been made, and the results are given.

Part II.—The different views as to the origin of uric acid in the animal body are discussed, and an endeavour is made to explain the various apparent difficulties of the subject. The following points are especially dwelt upon:—

(*a.*) The very varying amounts of uric acid thrown out by different animals in relation to their total nitrogenised elimination.

(*b.*) The excessively large excretion of uric acid by a great number of the lower animals, as birds, reptiles, and invertebrate animals, compared with the weight of their bodies. Under this head it is shown that whereas man excretes, on an average, $\frac{1}{150000}$th part of his weight of uric acid per diem, a bird often excretes as much as $\frac{1}{150}$th part of its weight during the same period; in other words, a bird throws out from its kidneys during a given time a thousand times more uric acid than a man.

The effect of uric acid and its salts upon the urinary excretion, when introduced into the animal economy, either with food or injected into the veins, is discussed, and it is shown that the kidneys do not possess the power of filtering uric acid from the blood when it is present in that fluid.

The different conditions in which uric acid is found in the kidneys and urinary excretions under varying circumstances, and also in the blood and tissues of the body, are dwelt upon.

The presence of uric acid in the urinary excretion of the young of the herbivorous mammal, and its usual absence in the case of the adult of the same species, are investigated; it is also shown that, whereas in the kidneys ammonia is combined with the acid, in the blood it is found as urate of sodium, as also when deposited in the different tissues of the body. This latter phenomenon is explained in full.

The normal presence of uric acid in the spleen, liver, and other organs, even of animals in which the urinary excretion is usually free from this principle, is fully discussed and explained.

An investigation into the mutually destructive influence which uric and hippuric acid exert upon each other is next detailed, and it is found that such action affords a clue to the solution of many of the difficulties of the subject.

The results of the whole investigation appear to show that uric acid is not, as is commonly supposed, formed in the animal body during the progress of the metabolism which is constantly going on in different organs and tissues, then thrown into the blood and afterwards filtered or strained off by the kidneys, and thus finally eliminated from the body; but that it is absolutely formed in the renal organs themselves by the action of peculiar cells; that it probably exists in these cells as the urate of an organic base yielding ammonia, or as a complex organic principle, readily splitting up into uric acid and ammonia; that, for the most part, it is excreted as such urate, which, however, may be changed into urate of sodium, or any other metallic urate, according as it subsequently meets with one or other salt; that, probably, there is always a trace of uric acid absorbed into the blood from the kidney-cells; but that, under certain circumstances, *e.g.*, when its forward progress is rendered difficult by tying the cloaca or the ureters of animals, or when other obstructive causes such as occur in disease, are at work, the absorption into the blood becomes greatly increased, and it is then converted into urate of sodium, on account of its then meeting with large amounts of the chloride and phosphate of that metal; and that it is at times deposited, both in man and the lower animals, in different structures, such as the cartilaginous and fibrous tissues, in the form of crystallised urate of sodium.

The Appendix contains the details of many experiments, especially in relation to the destructive influence of hippurates and benzoates upon uric acid; and of others which show the want of such power in the case of glycine, glucose, glycerine, and other substances.

March 1, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read from the Chair, as follows:—

Aitchison, James Edward Tierney, Surgeon-Major, M.D., F.R.C.S., F.R.S.E.
Allman, Professor George Johnston, LL.D.
Baird, A. W., Major R.E.
Baxendell, Joseph, F.R.A.S.
Bell, James, F.I.C.
Browne, James Crichton, M.D., LL.D., F.R.S.E.
Browne, Walter Raleigh, M.A., M.I.C.E.
Buchanan, Professor George, M.A., M.D.
Burdett, Henry Charles, F.L.S., F.S.S.
Colomb, Philip H., Captain R.N.
Creak, Ettrick William, Staff Commander R.N.
Cunningham, Allan Joseph Champneys, Major R.E.
Curtis, Arthur Hill, A.M., LL.D., D.Sc.
Dobson, George Edward, Surgeon-Major, M.A., M.B., F.L.S.
Duncan, James Matthews, A.M., M.D., LL.D.
Fitzgerald, Prof. George Francis, M.A.
Flight, Walter, D.Sc., F.G.S.
Foster, Professor Balthazar Walter, F.R.C.P.
Frost, Rev. Percival, M.A.
Gill, David, LL.D., F.R.A.S.
Goodeve, Professor Thomas Minchin, M.A.
Groves, Charles Edward, F.C.S.
Grubb, Howard, F.R.A.S.
Herschel, Professor Alexander Stewart.
Hicks, Henry, M.D., F.G.S.
Hudleston, Wilfrid H., M.A., F.G.S., F.C.S.
Kent, William Saville, F.Z.S.
Langley, John Newport, M.A.
McKendrick, John G., M.D.
Meldola, Raphael, F.R.A.S., F.C.S.
Miller, Francis Bowyer, F.C.S.
Milne, Professor John, F.G.S.
Priestley, Professor William Overend, M.D., F.R.C.P.
Pritchard, Urban, M.D., F.R.C.S.
Ransome, Arthur, M.A., M.D.
Reinold, Professor Arnold William, M.A.
Rendel, George Wightwick, M.I.C.E.
Ringer, Professor Sydney, M.D.
Rodwell, George F., F.R.A.S., F.C.S.
Sanders, Alfred, M.R.C.S., F.L.S.
Tenison-Woods, Rev. Julian E., M.A., F.L.S., F.G.S.
Tidy, Professor Charles Meymott, M.B., F.C.S.
Tribe, Alfred, F.C.S.
Trimen, Roland, F.L.S., F.Z.S.
Venn, John, M.A.

Walker, John James, M.A.
Warren, Charles, C.M.G., Major R.E.
Watson, Professor Morrison, M.D.
Williams, Charles Theodore, M.A., M.D., F.R.C.P.

The following Paper was read:—

I. "Contributions to the Chemistry of Storage Batteries." By E. FRANKLAND, D.C.L., F.R.S. Received February 21, 1883.

1. *Chemical Reactions.*—The chemical changes occurring during the charging and discharging of storage batteries have been the subject of considerable difference of opinion amongst chemists and physicists. Some writers believe that much of the storage effect depends upon the occlusion of oxygen and hydrogen gases by the positive and negative plates or by the active material thereon, some contend that lead sulphate plays an important part, whilst others assert that no chemical change of this sulphate occurs either in the charging or discharging of the plates.

To test the first of these opinions, I made two plates of strips of thin lead twisted into corkscrew form, and after filling the gutter of the screw with minium, so as to form a cylinder that could be afterwards introduced into a piece of combustion-tubing, these plates were immersed in dilute sulphuric acid and charged by the dynamo-current in the usual manner. The charging was continued until the whole of the minium on the + and − plates respectively was converted into lead peroxide and spongy lead, and until gas bubbles streamed from the pores of the two cylinders.

After removal from the acid the plates were superficially dried by filter-paper, and immediately introduced into separate pieces of combustion-tubing previously drawn out at one end, so as to form gas delivery tubes. The wide ends of these tubes were then sealed before the blowpipe, care being taken not to allow the heat to reach the enclosed cylinders. The tube containing the cylinder of reduced lead was now gradually heated until the lead melted, the drawn-out end of the tube meanwhile dipping into a pneumatic trough. The gas expelled from the tube consisted almost exclusively of the expanded air of the tube and contained mere traces of hydrogen.

The tube containing the cylinder of lead peroxide was similarly treated, except that the heat was not carried high enough to decompose the peroxide. Mere traces, if any, of occluded oxygen were evolved.

These results justify the conclusion that occluded gases play, practically, no part in the phenomena of the storage cell.

With regard to the function of lead sulphate in storage batteries,

I have observed that during the so-called "formation" of a storage cell, a very large amount of sulphuric acid disappears from the liquid contents of the cell: indeed sometimes the whole of it is withdrawn. The acid so removed must be employed in the formation of insoluble lead sulphate upon the plates which, in fact, soon become coated with a white deposit of the salt, formed equally upon both positive and negative surfaces. This visible deposit is, however, very superficial, and does not account for more than a very small fraction of the acid which actually disappears from solution. The great bulk of the lead sulphate cannot be discovered by the eye, owing to its admixture with chocolate-coloured lead peroxide.

Unless the coated plates have been previously immersed for several days in dilute sulphuric acid, this disappearance of acid during their "formation" continues for ten or twelve days. At length, however, as the charging goes on, the strength of the acid ceases to diminish and soon afterwards begins to augment. The increase continues until the maximum charge has been reached and abundance of oxygen and hydrogen gases begin to be discharged from the plates; that is to say, until the current is occupied exclusively, or nearly so, in the electrolysis of hexabasic sulphuric acid expressed by Burgoin in the following equation:—

$$\underset{\text{Sulphuric acid.}}{SO_6H_6} = \overbrace{\underset{\text{Sulphuric anhydride.}}{SO_3} + 3O}^{\text{Eliminated on + plate.}} + \overset{\text{Eliminated on − plate.}}{3H_2}.$$

Of course the sulphuric anhydride immediately combines with water and regenerates hexabasic sulphuric acid:—

$$SO_3 + 3OH_2 = SO_6H_6.$$

On discharging the cell, the specific gravity of the acid continually decreases until the discharge is finished, when it is found to have sunk to about the same point from which it began to increase during the charging. Hence it is evident that, during the discharge, the lead sulphate, which was continuously decomposed in charging, was continuously reformed in discharging.

The chief if not the only chemical changes occurring during the charging of a storage battery, therefore, appear to be the following:—

1st. The electrolysis of hexabasic sulphuric acid according to the equation already given.

2nd. The reconversion of sulphuric anhydride into sulphuric acid.

3rd. The chemical action on the coating of the + plate.

$$\underset{\text{Lead sulphate.}}{SO_4Pb} + O + 3OH_2 = \underset{\text{Lead peroxide.}}{PbO_2} + \underset{\text{Hexabasic sulphuric acid.}}{SO_6H_6}.$$

4th. The chemical action on the coating of the negative plate :—

$$\underset{\text{Lead sulphate.}}{SO_4Pb} + H_2 + 2OH_2 = Pb + \underset{\text{Hexabasic sulphuric acid.}}{SO_6H_6}.$$

If I have correctly described those changes, the initial action in the charging of a storage cell is the electrolysis of hexabasic sulphuric acid, each molecule of which throws upon the positive plate three atoms of oxygen, and upon the negative plate six atoms or three molecules of hydrogen. Each atom of oxygen decomposes one molecule of lead sulphate on the positive plate, producing one molecule of lead peroxide, and one of sulphuric anhydride, the latter instantly uniting with three molecules of water to form hexabasic sulphuric acid.

The following are the chemical changes which I conceive to occur during the discharge of a storage cell :—

1st. The electrolysis of hexabasic sulphuric acid as in charging.

2nd. The reconversion of sulphuric anhydride into hexabasic sulphuric acid as already described.

3rd. The chemical action upon the coating of what was before the positive plate or electrode, but which now becomes the negative plate of the cell, that is to say, the plate from which the positive current issues to the external circuit :—

$$\underset{\text{Lead peroxide.}}{PbO_2} + H_2 = \underset{\text{Lead oxide.}}{PbO} + \underset{\text{Water.}}{OH_2}.$$

The lead oxide thus formed is immediately converted into lead sulphate :—

$$PbO + SO_6H_6 = SO_4Pb + 3OH_2.$$

4th. The chemical action upon the coating of what has now become the positive plate of the cell :—

$$Pb + O + SO_6H_6 = SO_4Pb + 3OH_2.$$

Thus in discharging, as in charging, a storage cell, the initial action is the electrolysis of hexabasic sulphuric acid. The oxygen eliminated on the positive plate reconverts the reduced metal of that plate into lead oxide, whilst the hydrogen transforms the lead peroxide on the negative plate into the same oxide, which in both cases is immediately converted into lead sulphate by the surrounding sulphuric acid, thus restoring both plates to their original condition before the charging began.

The real "formation" of the cell consists, I conceive, in the more or less thorough decomposition of those portions of the lead sulphate which are comparatively remote from the conducting metallic nucleus of the plate. Lead sulphate itself has a very low conductivity, whilst lead peroxide, and especially spongy lead, offer comparatively little

resistance to the current, which is thus enabled to bring the outlying portions of the coating under its influence. It may be objected that, during the discharge, the work of formation would be undone; but probably, in the ordinary use of a storage battery, the discharge is never completed. Thus I have found that, in a small cell containing two plates 6″ × 2″, short circuiting with a thick copper wire for twelve hours was far from producing complete discharge, for on breaking this short circuit, the cell *instantly* rang violently an electric bell with which it was previously connected. In ordinary discharges of "formed" cells, therefore, the lead sulphate on the positive and negative plates still remains mixed with sufficient lead oxide and spongy lead respectively to give it a higher conducting power than the sulphate alone possesses.

2. *Chemical Estimation of the Charge in a Storage Cell.*—No method has hitherto been known by which the charge in a storage cell could be ascertained without discharging the cell; but the results of the foregoing experiments indicate a very simple means of ascertaining the amount of stored energy without any interference with the charge itself. The specific gravity and consequent strength of the dilute sulphuric acid of a "formed" cell being known in its uncharged and also in its fully charged condition, it is only necessary to take the specific gravity of the acid at any time in order to ascertain the proportion of its full charge which the cell contains at that moment; and if the duty of the cell is known, the amount of energy stored will also be thereby indicated. In the case of the cell with which I have experimented, containing about seven quarts of dilute sulphuric acid, each increase of ·005 in the specific gravity of the dilute acid means a storage of energy equal to 20 ampères of current for one hour, obtainable on discharge.

I hope shortly to be able to express, in terms of current from the cell, the definite relation between the amount of energy stored and the weight of sulphuric acid liberated.

March 8, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Notes on the Absorption of Ultra-Violet Rays by various Substances." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received March 1, 1883.

The following notes contain some records of ultra-violet absorptions in addition to those which have been examined by Soret, Hartley, M. de Chardonnet, and other investigators. For these observations we have generally used the spark of an induction coil, with Leyden jar, between iron electrodes as the source of light. Occasionally we have used other electrodes, but the lines of iron are so multitudinous, and so closely set in a large part of the ultra-violet region of the spectrum, that they form almost a continuous spectrum, at the same time there are amongst them a sufficient number of breaks and conspicuous lines to serve as points of reference. The spectroscope has a single prism of quartz, and the telescopes have quartz lenses. The image of the spark was projected on to the slit of the spectroscope by a quartz lens, and the absorbent substances were interposed between the slit and the last-mentioned lens. The gases were held in tubes fitted, some with quartz, others with rock salt, plates on the ends; liquids in cells with quartz sides. The spectra were all photographed.

Chlorine in small quantity shows a single absorption band extending from about N (3580) to T (3020). As the quantity of chlorine is increased this band widens, expanding on both sides, but rather more rapidly on the less refrangible side. Different quantities of chlorine produced absorption from about H (3968) to wave-length 2755, from wave-length 4415 to 2665, and from wave-length 4650 to 2630. With the greatest quantity of chlorine tried the absorption did not extend above wave-length 2550.

Bromine vapour in small quantity absorbs light up to about L (3820), and is quite transparent above that. With larger quantity

the absorption increases, gradually extending with increase of bromine vapour from L to P (3360); and at the same time there is a gradually increasing general absorption at the most refrangible end of the spectrum beginning at about wave-length 2500; so that the denser bromine vapour is transparent for a band between wave-length 2500 and 3350.

Liquid bromine in very thin film between two quartz plates is transparent for a band between wave-length about 3650 and 3400, shading away on both sides, so that below M on one side and above P on the other the absorption seems complete. The transparency of the liquid film ends on the more refrangible side just where that of the vapour begins.

Iodine vapour tolerably dense cuts off all within the range of our photographs below wave-length 4300, and its absorption gradually diminishes from that point up to about wave-length 4080, from that point it is transparent.* Denser vapour produces complete absorption up to 4080 and partial absorption above that point.

Iodine dissolved in carbon disulphide is transparent for a band between G and H, cutting off all above and below. It is not possible to tell how much of the light above M (3727) is absorbed by iodine in such a solution, inasmuch as carbon disulphide is opaque for rays more refrangible than M.

Iodine dissolved in carbon tetrachloride when the solution is weak, shows only the absorption due to the solvent, described below. More iodine increases the absorption until it is complete above P (3360), with shading edge as far down as about wave-length 3400.

Sulphurous acid gas produces an absorption band which is very marked between R (3179) and wave-length 2630, and a fainter absorption extending on the less refrangible side to O (3440), and on the other side to the end of the range photographed, wave-length 2300.

Sulphuretted hydrogen produces complete absorption above wave-length 2580. Below that a partial general absorption.

Vapour of carbon disulphide in very small quantity produces an absorption band extending from P to T, shading away at each end; no absorption in the higher region. With more vapour the absorption band widens, extending from about wave-length 3400 to 3000, and a second absorption occurs beginning at about wave-length 2580, and extending to the end of the range photographed.

Carbon tetrachloride liquid produces an absorption band with a maximum about R, extending, but with decreasing intensity, up to

* The principal absorption band of the haloids seems to shift towards the less refrangible side with increase of atomic weight, and so to agree with the general rule which Lecoq de Boisbaudran has noticed in the shifting of corresponding lines in the spectra of groups of similar metals.—March 16.

Q (3285) on one side, and to *s* (3045) on the other. In the higher region there is a second absorption sensible about wave-length 2600, and increasing in intensity up to about wave-length 2580, beyond which point it is complete.

Chlorine peroxide gives a succession of nine shaded bands, at nearly equal intervals, between M and S, with faint indications of others beyond. In the highest region this gas seems quite transparent.

A slice of chrome-alum a quarter of an inch thick, is transparent between wave-lengths 3270 and 2830, its absorption gradually increases on both sides of those limits, but rather more rapidly on the more refrangible side than on the other, and becomes complete below about wave-length 3360 and above wave-length 2730.

A very thin plate of mica shows absorption beginning about S (3100), rapidly increasing above U (2947), and complete above wave-length 2840.

A thin film of silver precipitated chemically on a plate of quartz transmits well a band of light between wave-length about 3350 and 3070, but is quite opaque beyond those limits on both sides.*

A thin film of gold similarly precipitated merely produces a slight general absorption all along the spectrum.

The difference between the limits of transparency of Iceland spar for the ordinary and extraordinary rays, inferred from theory by Lommel, we find to be very small, and hardly to be detected without using a considerable thickness, three inches or more, of the spar.

We had expected to be able to apply the well-known photometric method by means of polarised light to the comparison of intensities of ultra-violet rays. Ordinary Nicol's prisms are not applicable to ultra-violet rays on account of the opacity of the Canada balsam, with which they are cemented, but through the kindness of the President of the Society, we obtained from him the loan of a pair of Foucault's prisms. Upon taking photographs of the spectrum of the iron spark through this pair of prisms at various inclinations between the planes of polarisation of the two prisms, we found that for the whole range between the position of parallelism and the inclination of 80° there was no sensible difference of effect upon the photographic plate, though the length of exposure was in all cases the same. For inclinations between 80° and 90° there was a sensible and increasing diminution in the photographic effect as the planes of polarisation of the polariser and analyser were more nearly at right

* Cornu has noticed ("Spectre Normal du Soleil," p. 28, note) that such a film of silver is transparent for certain ultra-violet rays, but he places them about wave-length 270, which does not agree with our observations. Chardonnet ("Comp. rend.," February, 1883) says that the transparent band extends from O to S. W. A. Miller ("Phil. Trans.," 1863) noticed that for a certain distance in the ultra-violet a silver reflector did not reflect the incident rays.

angles to one another. It seems to follow from this that the full photographic effect on the dry gelatine plates used by us ensues when the intensity of the light reaches a certain limit, but that for intensities of light beyond that limit there is no sensible increase in the effect until the stage of solarisation is reached.

II. "Note on the Reversal of Hydrogen Lines; and on the Outburst of Hydrogen Lines when Water is dropped into the Arc." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received March 1, 1883.

The concentration of the radiation of hydrogen in a small number of spectral lines would lead us to expect that the absorption of light of the same refrangibility as those lines would, at the temperature of incandescence, be correspondingly strong, and that therefore the hydrogen lines would be easily reversed. The mass of hydrogen which we can raise to a temperature high enough to show the lines is, however, so small, that notwithstanding the great absorptive power of hydrogen for the rays which it emits, the reversal of the lines has not hitherto been noticed. We find, in fact, that the lines are very readily reversed, and the reversal may be easily observed.

When a short induction spark is taken between electrodes of aluminium or magnesium in hydrogen at atmospheric pressure, a large Leyden jar being connected with the secondary wire of the coil, the hydrogen lines show no reversal; but if the pressure of the hydrogen be increased by half an atmosphere or even less,* the lines expand and a fine dark line may be seen in the middle of the F line. As the pressure is increased this dark line becomes stronger, so that at two atmospheres it is very decided. As the F line expands with increase of pressure the dark line expands too and becomes a band. It is best seen when the pressure is between two and three atmospheres. When the pressure is further increased the dark band becomes diffuse, and at five atmospheres cannot be distinctly traced. No definite reversal of the C line was observed under these circumstances. The dispersion used, however, was only that of one prism.

By using a higher dispersion the reversal of both the C and F lines may be observed at lower pressures. For this purpose we have used a Plücker tube, filled with hydrogen and only exhausted until the spark would pass readily when a large jar was used.

* The pressures here mentioned are only measured by a metallic gauge attached to the Cailletet pump employed, and must therefore be only taken as approximately correct.

The light of the narrow part of the tube is, under these circumstances, very brilliant, while the spark in the broad ends is wider and less bright, but does not fill the tube. On viewing such a tube end on, and projecting the image of the narrow part of the tube on to the slit of the spectroscope, a continuous spectrum, of the width of the image of the narrow part of the tube, is seen, besides the lines of hydrogen given by the discharge in the wide part of the tube. These lines extend above and below the narrow continuous spectrum if the electrode is well placed so that half-an-inch or so of the spark in the wide part of the tube may intervene between the narrow part of the tube and the spectroscope. The continuous spectrum of the narrow part of the tube seems due chiefly to the expansion of the hydrogen lines when the discharge occurs in so confined a space, and it is much brighter than the lines given by the spark in the wide part of the tube. Where the latter cross the continuous spectrum a very evident absorption occurs. We have observed it with a diffraction grating. The C line in the third order falls so near the F line in the fourth, that both may be observed together. The appearance presented in our spectroscope is shown in the accompanying drawing; F is much more expanded than C, and the reversal consequently less marked though quite plain. The other lines being still more diffuse their absorption could not be traced.

We have before observed ("Proc. Roy. Soc.," vol. 30, p. 157) that the C and F lines of hydrogen are visible in the arc of a De Meritens magneto-electric machine taken in hydrogen; though in the arc of a Siemens machine the C line can only be detected at the instant of breaking the arc, the F line hardly at all. When, instead of taking the arc in hydrogen, small drops of water are allowed to fall from a fine pipette into the arc taken in air in a lime crucible, each drop as it falls into the arc produces an explosive outburst of the hydrogen lines. Generally the outburst is only momentary, but occasionally a sort of flickering arc is maintained for a second or two and the hydrogen line C is visible all the time. The lines (C and F) are usually much expanded, but are frequently very unequally wide in different parts of the line. F is weaker, more diffuse, and more difficult to see than C, and is visible for a shorter time. There is no sign of reversal. In the explosive character of the outburst and the

irregularity in the width of the lines, the effect resembles that of an outburst of hydrogen in the solar atmosphere. The elements of the water are, as we must suppose, separated in the arc, but from the explosive character of the effect they are not uniformly distributed in the arc. The arc being horizontal and the image of it projected on to the slit of the spectroscope, it was really a very small section of the arc which was under observation, and this renders the variation in the width of the lines the more remarkable.

III. "Note on the Order of Reversibility of the Lithium Lines." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received March 1, 1883.

In our communications on the reversal of the lines of metallic vapours, we have several times noticed ("Proc. Roy. Soc.," vol. 28, pp. 357, 369, 473) the reversal of the lithium lines, and concluded that the blue line is more easily reversed than the orange line. This, however, does not appear to be really the case. When much lithium is introduced into the arc, a second blue line is developed close to but slightly more refrangible than the well-known blue line. This second blue line produces with the other the appearance of a reversal, which deceived us until we became aware of the existence of the second line. The blue line (wave-length 4604) is really reversed without difficulty when sufficient lithium is present, but under these circumstances the orange line is also reversed. The latter line is also the one which first (of the two) shows reversal, and also the one which is more persistently reversed. Hence we place the lines in order of reversibility as follows: red, orange, blue, green, violet.

March 15, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Joseph Chamberlain was admitted into the Society.

The following Papers were read:—

I. "On the Changes which take place in the Deviations of the Standard Compass in the Iron Armour-plated, Iron, and Composite-built Ships of the Royal Navy on a considerable Change of Magnetic Latitude." By Staff-Commander E. W. CREAK, R.N., of the Admiralty Compass Department. Communicated by Captain Sir FREDERICK J. O. EVANS, R.N., K.C.B., F.R.S., Hydrographer of the Admiralty. Received March 1, 1883.

(Abstract.)

The period comprised between the years 1855–68 was one of active research into the magnetic character of the armour-plated and other ships of the Royal Navy, and iron ships of the Mercantile Navy.

Among other contributions to this subject, a paper by F. J. O. Evans, Esq., Staff Commander R.N., F.R.S., and Archibald Smith, Esq., F.R.S., was read before the Royal Society in March, 1865, relating to the armour-plated ships of the Royal Navy, and containing the first published results of the system of observation and analysis of the deviations of the compass established four years previously.

From lack of observations in widely different magnetic latitudes, the authors of that paper were unable to define the proportions of the semicircular deviation arising from vertical induction in soft iron, and that arising from the permanent or sub-permanent magnetism in hard iron.

During the last fifteen years vessels of all classes—except turret ships—have visited places of high southern magnetic inclination or dip, and the analysis of the deviation of their standard compasses has been made, showing the constants for hard and soft iron producing semicircular deviation.

The constants for soft iron provide a means of predicting probable changes of deviation on change of magnetic latitude for certain vessels of the following classes, and others of similar construction:—

1. Iron armour-plated.
2. Iron cased with wood.
3. Iron troop ships.
4. Steel and iron ships cased with wood.
5. Composite-built vessels.
6. Wooden ships with iron beams and vertical bulkheads.

These vessels were all in a state of magnetic stability previous to the observations which have been discussed, and their compasses have had the semicircular deviation reduced to small values, or corrected in England by permanent bar magnets.

This correction may be considered as the introduction of a permanent magnetic force acting independently, and in opposition to the magnetic forces of the ship proceeding from hard iron.

It is now proposed to consider the effects of a change of magnetic latitude on the component parts of the deviation.

Semicircular Deviation.

On semicircular deviation from fore and aft forces time has but little effect, and the greater part of it is due to permanent magnetism in hard iron, which may be reduced to zero for all latitudes by a permanent magnet.

A second but small part of the semicircular deviation proceeds from sub-permanent magnetism in hard iron. It is subject to alterations slowly by time, from concussion, and from the ship remaining in a constant position with respect to the magnetic meridian for several days, and is more intensely affected by a combination of the two latter causes.

Deviations from sub-permanent magnetism which have been temporarily altered in value as described, return slowly to their original value on removal of the inducing cause.

The principal cause of change in the semicircular deviation on change of magnetic latitude in corrected compasses, arises from vertical induction in soft iron which changes directly as the tangent of the dip.

In standard compasses judiciously placed with regard to surrounding iron, this element of change is small, and similar in value for similar classes of ships.

With very few exceptions nearly the whole of the semicircular deviation from transverse forces is due to permanent magnetism in hard iron subject to the same laws as that proceeding from fore and aft forces.

In the exceptional cases alluded to, there is a small part due to

vertical induction in soft iron, changing directly as the tangent of the dip.

Quadrantal Deviation.

This deviation, caused by induction in horizontal soft iron symmetrically placed, does not change with a change of magnetic latitude. Time alone appears to produce a gradual change in its value during the first two or three years after the ship is launched, when it becomes nearly permanent.

The diminution of the mean directive force of the needle, which is common to all modern vessels of war, improves slowly at first by lapse of time, and finally assumes a permanent value.

Relative Proportions of Hard and Soft Iron.

It has been found that the relative proportions of the hard and soft iron affecting the standard compasses of twenty-five vessels examined differ considerably, even in ships of similar construction.

This difference may be accounted for by the compasses not being placed in the same relative position in the ships considered as magnets of various forms, and containing numerous iron bodies introduced during equipment.

General Conclusions.

The following general conclusions have special reference to the standard compass of the six classes of vessels previously mentioned:—

1. A large proportion of the semicircular deviation is due to permanent magnetism in hard iron.

2. A large proportion of the semicircular deviation may be reduced to zero, or corrected for all magnetic latitudes, by fixing a hard steel bar magnet or magnets in the compass pillar in opposition to and of equal force to the forces producing that deviation.

3. A very small proportion of the semicircular deviation is due to sub-permanent magnetism, which diminishes slowly by lapse of time.

4. The sub-permanent magnetism produces deviation in the same direction as the permanent magnetism in hard iron, except when temporarily disturbed, (1) by the ship remaining in a constant position with respect to the magnetic meridian for several days, (2) by concussion, (3) or by both combined, when the disturbance is intensified.

5. To ascertain the full value of changes in the sub-permanent magnetism, observations should be taken immediately on the removal of the inducing cause.

6. In the usual place of the standard compass the deviation caused

by transient vertical induction in soft iron is small, and of the same value (nearly) for ships of similar construction.

7. The preceding conclusions point to the conditions which should govern the selection of a suitable position for the standard compass with regard to surrounding iron in the ship.

II. "Atmospheric Absorption in the Infra-Red of the Solar Spectrum." By Captain ABNEY, R.E., F.R.S., and Lieut.-Colonel FESTING, R.E. Received March 5, 1883.

Any investigations on the subject of atmospheric absorption are of such importance in the study of meteorology, that we have deemed it advisable to present a preliminary notice of certain results obtained by us, without waiting to present a more detailed account which will be communicated at a future date. From 1874, when one of us commenced photographing the spectrum in the above region, till more than a year ago, the extremely various manners in which the absorptions took place caused considerable perplexity as to their origin, and it was only after we had completed our paper on the absorption of certain liquids,* that a clue to the phenomena was apparently found. Since that time we have carefully watched the spectrum in relation to atmospheric moisture, and we think that more than a year's observations in London, when taken in connexion with a month's work, at an altitude of 8,500 feet on the Riffel, justify the conclusions we now lay before the Society.

A study of the map of the infra-red region of the solar spectrum,† and more especially a new and much more complete one, which is being prepared for presentation to the Royal Society by one of us, shows that the spectrum in this part is traversed by absorption lines of varying intensity. Besides these linear absorptions, photographs taken on days of different atmospheric conditions, show banded absorptions superposed over them. These latter are step by step absorptions increasing in intensity as they approach the limit of the spectrum at the least refrangible end. In the annexed diagram,‡ fig. 4 shows the general appearance of this region up to λ 10,000 on a fairly dry day: the banded absorption is small, taking place principally between λ 9420 and λ 9800: a trace of absorption is also visible between λ 8330 and λ 9420. On a cold day, with a north-easterly

* "The Influence of the Atomic Groupings of the Molecules of Organic Bodies on their Absorption in the Infra-Red Region of the Spectrum." "Phil. Trans.," Part III, 1881.

† "Phil. Trans.," 1880.

‡ The lines shown in the diagram are merely reference lines, and have nothing to do with the absorptions under consideration.

wind blowing, and also at a high altitude on a dry day, these absorptions nearly if not quite disappear. If we examine photographs taken when the air is nearly saturated with moisture (in some form or another) we have a spectrum like fig. 1. Except with very prolonged exposure no trace of a spectrum below λ 8330 can be photographed. Fig. 2 shows the absorption bands, where there is a difference of about 3° between the wet and dry bulb, the latter standing at about 50°. It will be noticed that the spectrum extends to the limit of about λ 9420, when total absorption steps in and blocks out the rest of the spectrum. Fig. 3 shows the spectrum where the difference between the wet and the dry bulb is about 6°. Figs. 5 and 6 show the absorption of thicknesses of 1 foot and 3 inches of water respectively, where the source of light gives a continuous spectrum; $\frac{1}{8}$ inch water merely shows the absorption bands below 9420. It will be seen that there is an accurate coincidence between these "water bands" and the absorption bands seen in the solar spectrum, and hence we cannot but assume that there is a connexion one with the other. In fact, on a dry day it is only necessary to place varying thicknesses of water before the slit of the spectroscope and to photograph the solar spectrum through them, in order to reproduce the phenomena observed on days in which there is more or less moisture present in the atmosphere. It is quite easy to deduce the moisture present in atmosphere at certain temperatures by a study of the photographs. There does appear a difference, however, in the intensity of the banded absorptions in hot weather and in cold about up to 50°. In the former they are less marked when the degree of saturation and the length of atmosphere traversed are the same as in the latter.

The accepted view, we believe, of absorption of vapours is that they give linear absorptions in certain thicknesses, and as the thickness increases or the density becomes greater, the lines blacken, new lines appear, and gradually total absorption sets in in the region where the lines are most numerous and close. It is in the range of possibility that the presence of a small quantity of vapour might show itself as a haze over some region of the spectrum; if, however, the quantity was gradually increased the haze would give place to the lines, and the phenomena just described would be repeated. Suppose several localities of absorption to exist, the absorptive power of the vapour increasing the further the locality was situated down in the infra-red, it might happen that whilst one locality showed only a haze of absorption, one further down might show total absorption, some locality between these two should show linear absorption.

In the case of the absorptions in the solar spectrum we find a very different state of things existing. A comparison of the photographs taken in London on days of different dryness, and with those taken at

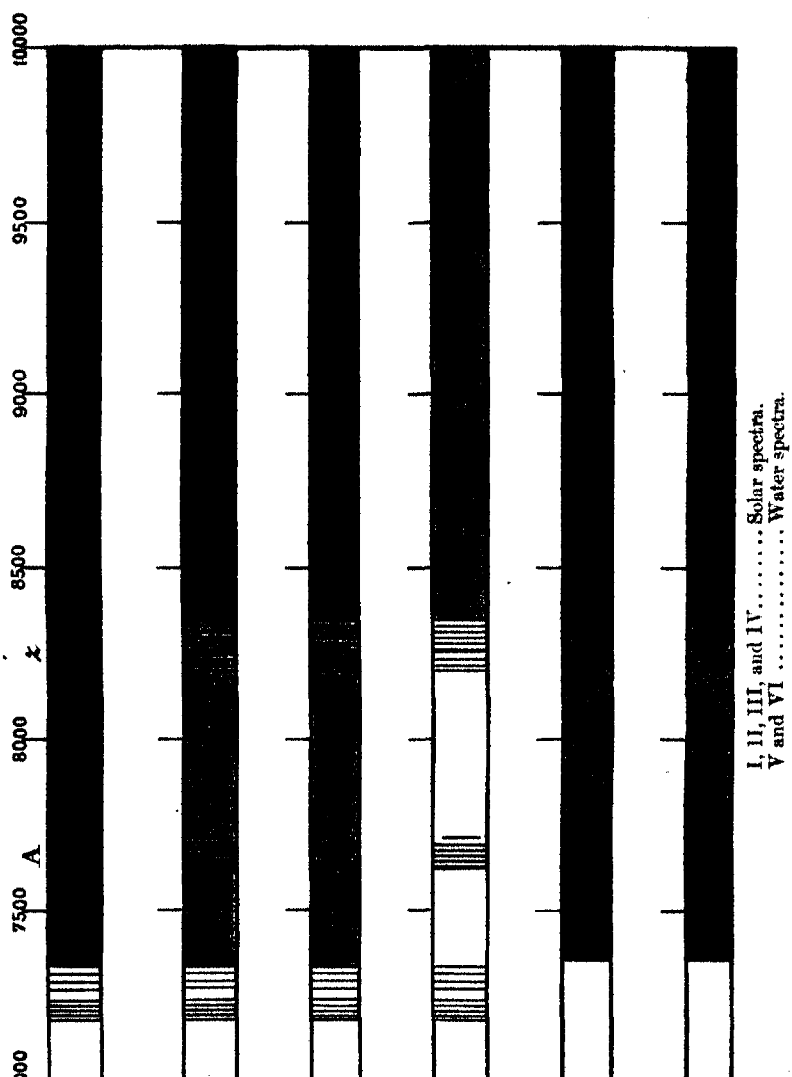


Fig. I. Fig. II. Fig. III. Fig. IV. Fig. V. Fig. VI.

the Riffel, shows that the linear absorptions are not increased in number or intensity; except so far that the blackness of the lines is increased by the blackness of the banded absorptions, and the same blackness can be induced by placing a certain thickness of water

before the slit of the spectroscope: another point is that the Fraunhofer lines in certain regions (say λ 9420 to λ 9800) are so irregularly distributed as to preclude the idea that they all belong to the absorption of aqueous vapour, yet all are equally darkened by the band, and they do not spread out as the darkness of the band increases. This is against the view of the bands being formed by aqueous vapour, as we know it.

The question then arises as to what those "water bands" can be due—if not due to vapour. This we consider an open question, and one which should be discussed. All we can state is that the absorptions shown are similar to those of water (liquid) and they do not seem to point to the watery stuff existing as vapour,* if we take the visible spectrum as a guide. An intense blue sky at sea-level is often indicative of moisture in the atmosphere, and it also seems to be indicative of finely suspended matter of some kind. If this be the case, can this suspended matter be suspended water stuff? for if it be not, there is no reason why the sky should be bluer on a moist day than on a dry day. We would remark that the deep blue sky at sea-level is of a different colour to the black-blue of high altitudes where, if they exist, the fine suspended particles would be largely diminished in number, and the coarser particles which cause white haze would also be fewer. The great difference of the intensities of the light from the blue sky in England and at 10,000 feet was determined by one of us and communicated to the British Association at Southampton, and the enormous disparity between the two has some bearing on the question we have been discussing.

Addendum.

In the above paper we have described the absorption due to "water stuff" in the atmosphere to λ 9800, as it is only to that wave-length to which the normal spectrum has been as yet published. We wish, however, to add that there are bands commencing at λ 9800, λ 12200, and λ 15200,† giving step by step absorption from the one wave-length to the next, as in the diagram, which also correspond with *cold* water bands. The absorption in the locality from 12200 downwards is usually total, and it is only on dry cold days or at high altitudes that we have noticed that rays of sufficient amplitude can penetrate to cause photographic impression to be made.—March 24, 1883.

* Unless it be held that the water itself holds vapour in solution.—March 12.

† These wave-lengths have been taken from the map of the prismatic spectrum illustrating the Bakerian Lecture, 1880, and are approximate numbers only.

III. "An Experimental Investigation of the Circumstances which Determine whether the Motion of Water shall be Direct or Sinuous, and of the Law of Resistance in Parallel Channels." By OSBORNE REYNOLDS, F.R.S. Received March 7, 1883.

(Abstract.)

1. *Objects and Results of the Investigation.*—The results of this investigation have both a practical and a philosophical aspect.

In their practical aspect they relate to the *laws of resistance to the motion of water in pipes*, which appears in a new form, the law for all velocities and all diameters being represented by an equation of two terms.

In their philosophical aspect these results relate to the fundamental principles of fluid motion; inasmuch as they afford for the case of pipes a definite verification of two principles, which are *that the general character of the motion of fluids in contact with solid surfaces depends on the relation* (1) *between the dimensions of the space occupied by the fluid and a linear physical constant of the fluid;* (2) *between the velocity and a physical velocity constant of the fluid.*

The results as viewed in their philosophical aspect were the primary object of the investigation.

As regards the practical aspect of the results, it is not necessary to say anything by way of introduction; but in order to render the philosophical scope and purpose of the investigation intelligible, it is necessary to describe shortly the line of reasoning which determined the order of investigation.

2. *The Leading Features of the Motion of Actual Fluids.*—Although in most ways the exact manner in which water moves is difficult to perceive, and still more difficult to define, as are also the forces attending such motion, certain general features both of the forces and motions stand prominently forth as if to invite or defy theoretical treatment.

The relations between the resistance encountered by, and the velocity of a solid body moving steadily through a fluid in which it is completely immersed, or of water moving through a tube, present themselves mostly in one or other of two simple forms. The resistance is generally proportional to the square of the velocity, and when this is not the case it takes a simpler form, and is proportional to the velocity.

Again, the internal motion of water assumes one or other of two broadly distinguishable forms—either the elements of the fluid follow one another along lines of motion which lead in the most direct manner to their destination, or they eddy about in sinuous paths, the most indirect possible.

The transparency or uniform opacity of most fluids renders it impossible to see the internal motion, so that, broadly distinct as are the two classes (direct and sinuous) of motion, their existence would not have been perceived, were it not that the surface of water, where otherwise undisturbed, indicates the nature of the motion beneath. A clean surface of moving water has two appearances, the one like that of *plate glass* in which objects are reflected without distortion; the other like that of *sheet glass*, in which the reflected objects appear crumpled up and grimacing. These two characters of surface correspond to the two characters of motion. This may be shown by adding a few streaks of highly coloured water to the clear moving water. Then, although the coloured streaks may at first be irregular they will, if there are no eddies, soon be drawn out into even colour bands; whereas if there are eddies, they will be curled and whirled about in the manner so familiar with smoke.

3. *Connexion between the Leading Features of Fluid Motion.*—These leading features of fluid motion are well known, and are supposed to be more or less connected, but it does not appear that hitherto any very determined efforts have been made to trace a definite connexion between them, or to trace the characteristics of the circumstances under which they are usually presented.

Certain circumstances have been definitely associated with the particular laws of force. Resistance as the square of the velocity is associated with motion in tubes of more than capillary dimensions, and with the motion of the bodies through the water at more than insensibly small velocities, while resistance as the velocity is associated with capillary tubes and small velocities.

The equations of hydrodynamics, although they are applicable to *direct motion, i.e.*, without eddies, and show that then the resistance is as the velocity, have hitherto thrown no light on the circumstances on which such motion depends. And although of late years these equations have been applied to the theory of the eddy, they have not been in the least applied to the motion of water, which is a mass of eddies, *i.e.*, in *sinuous motion*, nor have they yielded a clue to the cause of resistance varying as the square of the velocity. Thus, while as applied to waves and the motion of water in capillary tubes the theoretical results agree with the experimental, the theory of hydrodynamics has so far failed to afford the slightest hint why it should explain these phenomena, and signally failed to explain the law of resistance encountered by large bodies moving at sensibly high velocities through water, or that of water in sensibly large pipes.

This accidental fitness of the theory to explain certain of the phenomena, while entirely failing to explain others, affords strong presumption that there are some fundamental principles of fluid motion of which due account has not been taken in the theory; and

several years ago it seemed to me that a careful examination as to the connexion between these four leading features, together with the circumstances on which they severally depend, was the most likely means of finding the clue to the principles overlooked.

4. *Space and Velocity.*—The definite association of resistance as the square of the velocity with sensibly large tubes and high velocities, and of resistance as the velocity with capillary tubes and slow velocities, seemed to be evidence of the very general and important influence of some properties of fluids not recognised in the theory of hydrodynamics.

As there is no such thing as absolute space or absolute time recognised in mechanical philosophy, to suppose that the character of motion of fluids in any way depended on absolute size or absolute velocity would be to suppose such motion outside the pale of the laws of motion. If, then, fluids, in their motions, are subject to these laws, what appears to be the dependence of the character of the motion on the absolute size of the tube and on the absolute velocity of the immersed body must in reality be a dependence on the size of the tube as compared with the size of some other object, and on the velocity of the body as compared with some other velocity. What is the standard object and what the standard velocity which come into comparison with the size of the tube and the velocity of an immersed body, are questions to which the answers were not obvious. Answers, however, were found in the discovery of a circumstance on which sinuous motion depends.

5. *The Effect of Viscosity on the Character of Fluid Motion.*—The small evidence which clear water shows as to the existence of internal eddies, not less than the difficulty of estimating the viscous nature of the fluid, appears to have hitherto obscured the very important circumstance that *the more viscous a fluid is the less prone is it to eddying or sinuous motion.* To express this definitely, if μ is the viscosity and ρ the density of the fluid, for water $\frac{\mu}{\rho}$ diminishes rapidly as the temperature rises; thus at 5° C. $\frac{\mu}{\rho}$ is double what it is at 45° C. What I observed was that the tendency of water to eddy becomes much greater as the temperature rises.

Hence, connecting the change in the law of resistance with the birth and development of eddies, this discovery limited further search for the standard distance and standard velocity to the physical properties of the fluid.

To follow the line of this search would be to enter upon a molecular theory of liquids, and this is beyond my present purpose. It is sufficient here to notice the well known fact that—

is a quantity of the nature of the product of a distance and a velocity; and to point out that the establishment of a dependence of the character of fluid motion on a relation between the linear size of the space, the velocity of the fluid, and $\frac{\mu}{\rho}$, would be equivalent to establishing the existence of two physical constants, one a distance and the other a velocity or a time, as amongst the properties of fluids. Using the term dimension as implying measures of time as well as space, these constants may well be called dimensional properties or fluids. Similar constants are already recognised; thus the velocity of sound is such a velocity constant, and the mean paths of gaseous molecules, or the mean range, are such linear constants.

It is always difficult to trace the dependence of one idea on another; but it may be noticed that no idea of dimensional properties, as indicated by the dependence of the character of motion on the size of the tube and the velocity of the fluid, occurred to me until after the completion of my investigation on the transpiration of gases, in which was established the dependence of the law of transpiration on the relation between the size of the channel and the *mean range* of the gaseous molecules.

6. *Evidence of Dimensional Properties in the Equations of Motion.*— The equations of motion had been subjected to such close scrutiny, particularly by Professor Stokes, that there was small chance of discovering anything new or faulty in them. It seemed to me possible, however, that they might contain evidence which had been overlooked, of the dependence of the character of motion on a relation between the dimensional properties and the external circumstances of motion. Such evidence, not only of a connexion, but of a definite connexion, was found, and this without integration.

If the motion be supposed to depend on a single velocity parameter U—say the mean velocity along a tube—and on a single linear parameter c, say the radius of the tube; then, having in the usual manner eliminated the pressure from the equations, there remain two types of terms in one of which—

$$\frac{U^2}{c^3}$$

is a factor, and in the other—

$$\frac{\mu U}{\rho c^4}$$

is a factor. So that the relative values of these terms vary respectively as U and—

$$\frac{\mu}{c\rho}.$$

This is a definite relation of the exact kind for which I was in

search. Of course, without integration the equations only gave the relation, without showing at all in what way the motion might depend upon it. It seemed, however, to be certain, if the eddies were owing to one particular cause, that integration would show the birth of eddies to depend upon some definite value of—

$$\frac{c\rho U}{\mu}$$

7. *The Cause of Eddies.*—There appeared to be two possible causes for the change of direct motion into sinuous. These are best discussed in the language of hydrodynamics; but as the results of this investigation relate to both these causes, which, although the distinction is subtile, are fundamentally distinct and lead to distinct results, it is necessary that they should be indicated.

The general cause of the change from steady to eddying motion was, in 1843, pointed out by Professor Stokes as being that, under certain circumstances, the steady motion becomes unstable, so that an indefinitely small disturbance may lead to a change to sinuous motion. Both the causes above referred to are of this kind, and yet they are distinct; the distinction lying in the part taken in the instability by viscosity. If we imagine a fluid free from viscosity and absolutely free to glide over solid surfaces, then comparing such a fluid with a viscous fluid in exactly the same motion—

(1.) The frictionless fluid might be unstable and the viscous stable. Under these circumstances the cause of eddies is the instability as a perfect fluid, the effect of viscosity being in the direction of stability.

(2.) The frictionless fluid might be stable and the viscous fluid unstable; under which circumstances the cause of instability would be the viscosity.

It was clear to me that the conclusion I had drawn from the equations of motion immediately related only to the first cause. Nor could I then perceive any possible way in which instability could result from viscosity. All the same I felt a certain amount of uncertainty in assuming the first cause of instability to be general. This uncertainty was the result of various considerations, but particularly from my having observed that eddies apparently come on in very different ways, according to a very definite circumstance of motion, which may be illustrated.

When in a channel the water is all moving in the same direction, the velocity being greatest in the middle and diminishing to zero at the sides, as indicated by the curve in fig. 1, eddies showed themselves reluctantly and irregularly; whereas when the water on one side of the channel was moving in the opposite direction to that on

FIG. 1.

the other, as shown by the curve in fig. 2, eddies appeared in the middle regularly and readily.

FIG. 2.

8. *Methods of Investigation.*—There appeared to be two ways of proceeding, the one theoretical, the other practical.

The theoretical method involved the integration of equations for unsteady motion in a way that had not then been accomplished, and which, considering the general intractability of the equations, was not promising.

The practical method was to test the relation between U, $\frac{\mu}{\rho}$, and c; this, owing to the simple and definite form of the law, seemed to offer, at all events in the first place, a far more promising field of research.

The law of motion in a straight smooth tube offered the simplest possible circumstances and the most crucial test.

The existing experimental knowledge of the resistance of water in tubes, although very extensive, was in one important respect incomplete. The previous experiments might be divided into two classes—(1) those made under circumstances in which the law of résistance was as the square of the velocity, and (2) those made under circumstances in which the resistance varied as the velocity. There had not apparently been any attempt made to determine the exact circumstances under which the change of law took place.

Again, although it had been definitely pointed out that eddies would explain the resistance as the square of the velocity, it did not appear that any definite experimental evidence of the existence of eddies in parallel tubes had been obtained, and much less was there

any evidence as to whether the birth of eddies was simultaneous with the change in the law of resistance.

These open points may be best expressed in the form of queries to which the answers anticipated were in the affirmative.

(1.) What was the exact relation between the diameters of the pipes and the velocities of the water at which the law of resistance changed; was it at a certain value of

$$cU?$$

(2.) Did this change depend on the temperature, *i.e.*, the viscosity of water; was it at a certain value of

$$\frac{U}{\mu}?$$

(3.) Were there eddies in parallel tubes?

(4.) Did steady motion hold up to a critical value and then eddies come in?

(5.) Did the eddies come in at a certain value of

$$\frac{\rho cU}{\mu}?$$

(6.) Did the eddies first make their appearance as small, and then increase gradually with the velocity, or did they come in suddenly?

The bearing of the last query may not be obvious; but, as will appear in the sequel, its importance was such that in spite of satisfactory answers to all the other queries, a negative answer to this in respect of one particular class of motions led to the reconsideration of the supposed cause of instability and eventually to the discovery of instability caused by fluid friction.

The queries as they are put suggest two methods of experimenting:—

(1.) Measuring the resistances and velocities for different diameters, and with different temperatures of water.

(2.) Visual observation as to the appearance of eddies during the flow of water along tubes or open channels.

Both these methods have been adopted, but as the question relating to eddies had been the least studied the second method was the first adopted.

9. *Experiments by Visual Observations.*—The most important of these experiments related to water moving in one direction along glass tubes. Besides these, however, experiments on fluids flowing in opposite directions in the same tube were made; also a third class of experiments which related to motion in a flat channel of indefinite breadth.

These last-mentioned experiments resulted from an incidental observation during some experiments made in 1876 as to the effect

of oil to prevent wind waves. As the result of this observation had no small influence in directing the course of this investigation, it may be well to describe it first.

10. *Eddies caused by the Wind beneath the Oiled Surface of Water.*—A few drops of oil on the windward side of a pond during a stiff breeze having spread over the pond and completely calmed the surface as regards waves, the sheet of oil, if it may be so called, was observed to drift before the wind, and it was then particularly noticed that close to, and at a considerable distance from, the windward edge, the surface presented the appearance of *plate glass;* further from the edge the surface presented that wavering appearance which has already been likened to that of sheet glass, which appearance was at the time noted as showing the existence of eddies beneath the surface.

Subsequent observation confirmed this first view. At a sufficient distance from the windward edge of an oil-calmed surface there are always eddies beneath the surface even when the wind is light. But the distance from the edge increases rapidly as the force of the wind diminishes, so that at a limited distance (10 or 20 feet) the eddies will come and go with the wind.

Without oil I was unable to perceive any indication of eddies. At first I thought that the waves might prevent their appearance even if they were there, but by careful observation I convinced myself that they were not there. It is not necessary to discuss these results here, although, as will appear, they have a very important bearing on the cause of instability.

11. *Experiments by Means of Colour Bands in Glass Tubes.*—These were undertaken early in 1880; the final experiments were made on three tubes, Nos. 1, 2, and 3.

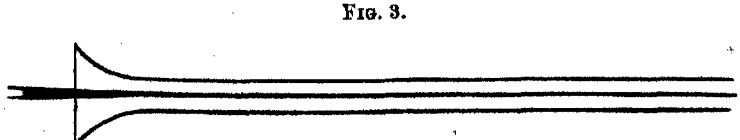
The diameters of these were nearly 1-inch, ½-inch, and ¼-inch. They were all about 4 feet 6 inches long, and fitted with trumpet mouthpieces, so that water might enter without disturbance.

The water was drawn through the tubes out of a large glass tank in which the tubes were immersed, arrangements being made so that a streak or streaks of highly coloured water entered the tubes with the clear water.

The general results were as follows:—

(1.) When the velocities were sufficiently low, the streak of colour extended in a beautiful straight line through the tube, fig. 3.

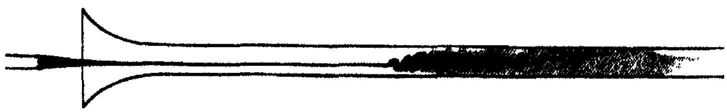
FIG. 3.



(2.) If the water in the tank had not quite settled to rest, at sufficiently low velocities the streak would shift about the tube, but there was no appearance of sinuosity.

(3.) As the velocity was increased by small stages at some point in the tube always at a considerable distance from the trumpet or intake, the colour band would all at once mix up with the surrounding water, and fill the rest of the tube with a mass of coloured water, as in fig. 4.

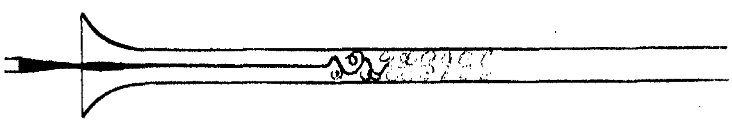
FIG. 4.



Any increase in the velocity caused the point of breakdown to approach the trumpet, but with no velocities that were tried did it reach this.

On viewing the tube by the light of an electric spark, the mass of colours resolved itself into a mass of more or less distinct curls showing eddies, as in fig. 5.

FIG. 5.



The experiments thus seemed to settle questions 3 and 4 in the affirmative—the existence of eddies and a critical velocity.

They also settled in the negative, question 6 as to the eddies coming in gradually after the critical velocity was reached.

In order to obtain an answer to question 5 as to the law of the critical velocity, the diameters of the tubes were carefully measured, also the temperature of the water and the rate of discharge.

(4.) It was then found that with water at a constant temperature and the tank as still as could by any means be brought about, the critical velocities at which the eddies showed themselves were exactly in the inverse ratios of the diameters of the tubes.

(5.) That in all the tubes the critical velocity diminished as the temperature increased, the range being from 5° C. to 22° C. and the law of this diminution, so far as could be determined, was in accordance with Poiseuille's experiment.

Taking T to express degrees Centigrade, then by Poiseuille's experiments—

$$\frac{\mu}{\rho} \propto P = 1 + 0{\cdot}0336\,T + 0{\cdot}00221\,T^2,$$

Taking a metre as the unit, U_s the critical velocity, and D the diameter of the tube, the law of the critical point is completely expressed by the formula

$$U_s = \frac{1}{B_s} \frac{P}{D}$$

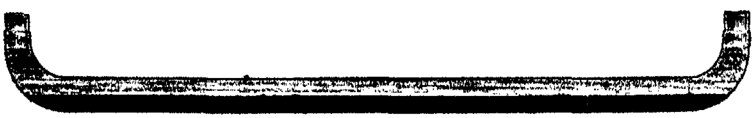
where $B_s = 43{\cdot}7$

This is a complete answer to question 5.

During the experiments many things were noticed which cannot be mentioned here, but two circumstances should be mentioned as emphasizing the negative answer to question 6. In the first place, the critical velocity was much higher than had been expected in pipes of such magnitude, resistance varying as the square of the velocity had been found at very much smaller velocities than those at which the eddies appeared when the water in the tank was steady. And in the second place it was observed that the critical velocity was very sensitive to disturbance in the water before entering the tubes, and it was only by the greatest care as to the uniformity of the temperature of the tank and the stillness of the water that consistent results were obtained. This showed that the steady motion was unstable for large disturbances long before the critical velocity was reached, a fact which agreed with the full blown manner in which the eddies appeared.

12. *Experiments with two Streams in Opposite Directions in the same Tube.*—A glass tube 5 feet long and 1·2 inch in diameter, having its ends slightly bent up as shown in fig 6, was half filled with bisulphide

FIG. 6.



of carbon, and then filled up with water and both ends corked. The bisulphide was chosen as being a limpid liquid, but little heavier than water and completely insoluble, the surface between the two liquids being clearly distinguishable. When the tube was placed in a horizontal direction, the weight of the bisulphide caused it to spread along the lower half of the tube, and the surface of separation of the two liquids extended along the axis of the tube.

On one end of the tube being slightly raised the water would flow to the upper end, and the bisulphide fall to the lower, causing opposite currents along the upper and lower halves of the tube, while in the middle of the tube the level of the surface of separation remained unaltered.

The particular purpose of this investigation was to ascertain whether there was a critical velocity at which waves or sinuosities would show themselves in the surface of separation. It proved a very pretty experiment and completely answered its purpose.

When one end was raised quickly by a definite amount, the opposite velocities of the two liquids, which were greatest in the middle of the tube, attained a certain maximum value depending on the inclination given to the tube. When this was small no signs of eddies or sinuosities showed themselves, but at a certain definite inclination waves (nearly stationary) showed themselves, presenting all the appearance of wind waves.

These waves first made their appearance as very small waves of equal lengths, the length being comparable to the diameter of the tube.

FIG. 7.

When by increasing the rise, the velocities of flow were increased, the waves kept the same length, but became higher, and when the rise was sufficient, the waves would curl and break, the one fluid winding itself into the other in regular eddies.

Whatever might be the cause, a skin formed slowly between the bisulphide and the water, and this skin produced similar effects to that of oil on water, the results mentioned are those which were obtained before the skin showed itself. When the skin first came on regular waves ceased to form, and in their place the surface was disturbed as if by irregular eddies above and below, just as in the case of the oiled surface of water.

The experiment was not adapted to afford a definite measure of the velocities at which the various phenomena occurred, but it was obvious that the critical velocity at which the waves first appeared, was many times smaller than the critical velocity in a tube of the same size when the motion was in one direction only. It was also clear that the critical velocity was nearly if not quite independent of any existing disturbance in the liquids. So that this experiment shows—

(1.) That there is a critical velocity in the case of opposite flow, at which direct motion becomes unstable.

(2.) That the instability came on gradually and did not depend on the magnitude of the disturbances, or in other words, that for this class of motion question 6 must be answered in the affirmative.

It thus appeared that there was some difference in the cause of instability in the two motions.

13. *Further Study of the Equations of Motion.*—Having now definite data to guide me, I was anxious to obtain a fuller explanation of these results from the equation of motion. I still saw only one way open to account for the instability, namely, by assuming the instability of a frictionless fluid to be general.

Having found a method of integrating the equations as far as to show whether any particular form of steady motion is stable for a small disturbance, I applied this method to the case of parallel flow in a frictionless fluid. The results which I obtained at once were, *that flow in one direction was stable, flow in opposite directions unstable.* This was not what I was looking for, and I spent much time in trying to find a way out of it, but whatever objections my method of integration may be open to, I could make nothing less of it.

It was not until the end of 1882 that I abandomed further attempts with a frictionless fluid and attempted by the same method the integration of a viscous fluid. This change was in consequence of a discovery that in previously considering the effect of viscosity I had omitted to take fully into account the boundary conditions which resulted from the friction between the fluid and the solid boundary.

On taking these boundary conditions into account, it appeared that although the tendency of viscosity through the fluid is to render direct or steady motion stable, yet owing to the boundary condition resulting from the friction at the solid surface, the motion of the fluid irrespective of viscosity would be unstable. Of course this cannot be rendered intelligible without going into mathematics. But what I want to point out is that this instability, as shown by the integration of the equations of motion, depends on exactly the same relation

$$U \propto \frac{\mu}{\rho c}$$

as that previously found.

This explained all the practical anomalies, and particularly the absence of eddies below a pure surface of water exposed to the wind; for in this case, the surface being free, the boundary condition was absent, whereas the film of oil by its tangential stiffness introduced this condition. This circumstance alone seemed a sufficient verification of the theoretical conclusion.

But there was also the sudden way in which eddies came into existence in the experiments with the colour band, and the effect of disturbances to lower the critical velocity. These were also explained, for as long as the motion was steady the instability depended upon the boundary action alone, but once eddies introduced the stability would be broken down.

It thus appeared that the meaning of the experimental results had

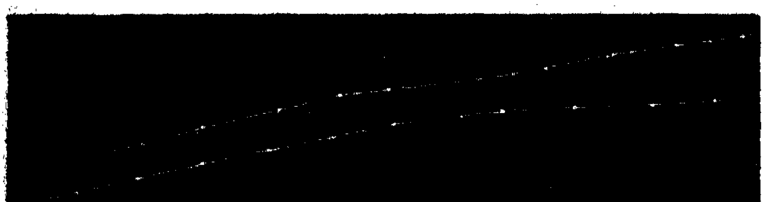
been ascertained, and the relation between the four leading features and the circumstances on which they depend traced, for the case of water in parallel flow. But as it appeared that the critical velocity in the case of motion in one direction did not depend on the cause of instability with a view to which it was investigated, it followed that there must be another critical velocity which would be the velocity at which previously existing eddies would die out, and the motion become steady as the water proceeded along the tube. This conclusion has been verified.

14. *Results of Experiments on the Law of Resistance in Tubes.*—The existence of the critical velocity described in the previous article could only be tested by allowing water in a high state of disturbance to enter a tube, and after flowing a sufficient distance for the eddies to die out, if they were going to die out, to test the motion. As it seemed impossible to apply the method of colour bands, the test applied was that of the law of resistance as indicated in questions (1) and (2) in § 8. The result was very happy. Two straight lead pipes, No. 4 and No. 5, each 16 feet long, and having diameters of a quarter and half inch respectively, were used.

The water was allowed to flow through rather more than 10 feet before coming to the first gauge-hole, the second gauge-hole being 5 feet further along the pipe.

The results were very definite, and are partly shown in fig. 8.

Fig. 8.



(1.) At the lower velocities the pressure was proportional to the velocity, and the velocities at which a deviation from this law first occurred were in the exact inverse ratio of the diameters of the pipes.

(2.) Up to these critical velocities the discharges from the pipes agreed exactly with those given by Poiseuille's formula for capillary tubes.

(3.) For some little distance after passing the critical velocity no very simple relations appeared to hold between the pressures and velocities; but by the time the velocity reached 1·3 (critical velocity) the relation became again simple. The pressure did not vary as the square of the velocity, but as 1·722 power of the velocity; this law

held in both tubes, and through velocities ranging from 1 to 50, where it showed no signs of breaking down.

(4.) The most striking result was that not only at the critical velocity, but throughout the entire motion the laws of resistance exactly corresponded for velocities in the ratio of

$$\frac{\mu}{\rho c}.$$

This last result was brought out in the most striking manner on reducing the results by the graphic method of logarithmic homologues as described in my paper on Thermal Transpiration.

Calling the resistance per unit of length as measured in the weight of cubic units of water i, and the velocity v, $\log i$ is taken for abscissa, and $\log v$ for ordinate, and the curve plotted.

In this way the experimental results for each tube are represented as a curve; these curves, which are shown as far as the small scale will admit in fig. 9, present exactly the same shape, and only differ in position.

FIG. 9.

Pipe.	Diameter. m.
No. 4, Lead........	0·00615
„ 5, „	0·0127
A, Glass	0·0496
B, Cast iron	0·188
D, „	0·5
C, Varnish	0·196

Either of the curves may be brought into exact coincidence with the other by a rectangular shift, and the horizontal shifts are given

by the difference of the logarithm of $\frac{D^3}{\mu^2}$ for the two tubes, the vertical shifts by the difference of the logarithm of

$$\frac{D}{\mu}.$$

The temperatures at which the experiment had been made were nearly the same, but not quite, so that the effect of the variations of μ showed themselves.

15. *Comparison with Darcy's Experiments.*—The definiteness of these results, their agreement with Poiseuille's law, and the new form which they more than indicated for the law of resistance above the critical velocity, led me to compare them with the well-known experiments of Darcy on pipes ranging from 0·014 to 0·5 metre. Taking no notice of the empirical laws by which Darcy had endeavoured to represent his results, I had the logarithmic homologues plotted from his published experiments. If my law was general then these log curves, together with mine, should all shift into coincidence if each were shifted horizontally through

$$\frac{D^3}{P^2}$$

and vertically through—

$$\frac{D}{P}.$$

In calculating these shifts there were some doubtful points. Darcy's pipes were not uniform between the gauge points, the sections varying as much as 20 per cent., and the temperature was only casually given. These matters rendered a close agreement unlikely; it was rather a question of seeing if there was any systematic disagreement. When the curves came to be shifted the agreement was remarkable; in only one respect was there any systematic disagreement, and this only raised another point; it was only in the slopes of the higher portions of the curves. In both my tubes the slopes were as 1·722 to 1; in Darcy's they varied according to the nature of the material, from the lead pipes, which were the same as mine, to 1·92 to 1 with the cast iron. This seems to show that the nature of the surface of the pipe has an effect on the law of resistance above the critical velocity.

16. *The Critical Velocities.*—All the experiments agreed in giving

$$v_c = \frac{1}{278}\frac{P}{D}$$

as the critical velocity, to which correspond as the critical pressure

$$i_c = \frac{1}{47700000}\frac{P^2}{D^3},$$

the units being metres and degrees Centigrade. It will be observed that this value is much less than the critical velocity at which steady motion broke down.

17. *General Law of Resistance.*—The log homologues all consist of two straight branches, the lower branch inclined at 45°, and the upper one at n horizontal to 1 vertical, except for the small distance beyond the critical velocity these branches constitute the curves. These two branches meet in a point o on the curve at a definite distance below the critical pressure, so that, ignoring the small portion of the curve above the point before it again coincides with the upper branch, the logarithmic homologues give for the law of resistance for all pipes and all velocities—

$$A\frac{D^3}{P^2}i=\left(B\frac{D}{P}v\right)^n,$$

where n has the value unity as long as either member is below unity, and then takes the value of the slope n to 1 for the particular surface of the pipe.

If the units are metres and degrees Centigrade—

$$A=67{,}700{,}000,$$
$$B=398,$$
$$P=1+0{\cdot}0336\,T+0{\cdot}000221\,T^2.$$

This equation then, excluding the region immediately about the critical velocity, gives the law of resistance in Poiseuille's tubes, those of the present investigation, and Darcy's, the range of diameters being

from 0·000013 (Poiseuille, 1843),

to 0·5 (Darcy, 1857);

and the range of velocities—

from 0·0026 }
to 7 } metres per sec., 1883.

This algebraical formula shows that the experiments entirely accord with the theoretical conclusions. The empirical constants are A, B, P, and n; the first three relate solely to the dimensional properties of the fluid which enter into the viscosity, and it seems probable that the last relates to the properties of the surface of the pipe.

Much of the success of the experiments is due to the care and skill of Mr. Foster of Owens College, who has constructed the apparatus and assisted me in making the experiments.

The Society then adjourned over the Easter Recess to Thursday, April 5th.

Presents, February 1, 1883.

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"On the Action of certain Reagents upon the Coloured Blood-Corpuscles. Part I. The Coloured Blood-Corpuscles of the Newt and Frog." By WILLIAM STIRLING, M.D., Sc.D., Professor of the Institutes of Medicine, and ARTHUR RANNIE, Graduate in Medicine of the University of Aberdeen. Communicated by Professor HUXLEY, F.R.S. Received June 14. Read June 15, 1882.

[PLATE 1.]

The histological and chemical constitution of the coloured blood-corpuscles of man and other animals has formed a fertile source of investigation for a large number of observers, and it might seem that further investigations on this subject were unnecessary. We have, however, devoted considerable time to a systematic study of the effects of certain reagents upon the blood of the newt and frog which have yielded results, some of them of not a little interest and importance.

The literature of the subject, chiefly of the German papers, is given somewhat fully in Rollett's article, "Blood," in Stricker's "Histology," and a careful *résumé* of most of the more recent and some of the older observations will be found in a short paper by G. F. Dowdeswell, in the "Quarterly Journal of Microscopical Science" for 1881.* Reference will be made to the literature of each reagent under its appropriate heading.

The *method* of conducting the investigation was as follows:—A newt was pithed; its heart exposed; the auricle snipped through and the blood collected.† A drop of this blood was then placed on a slide, and covered with a cover-glass, a hair having first been placed between the cover-glass and the slide to admit of the corpuscles rolling freely over and over so as to be seen on edge as well as on the flat, and also to allow the corpuscles to expand freely under the influence of reagents. The blood was then irrigated with a solution of the reagent to be investigated and examined with a magnifying power of 300 diameters, or a higher magnifying power when this was deemed desirable. Similar experiments were made with frog's blood, but newt's blood was preferred on account of the corpuscles being larger.

Pyrogallic Acid.

On irrigating a drop of the blood with a 2 per cent. solution of

* An excellent digest up to date is given by Professor Lankester in his exhaustive paper "Observations and Experiments on the Red Blood-Corpuscle," in the same Journal for the year 1871.

† Results were obtained equally with defibrinated and ordinary blood.

pyrogallic acid—which had become yellow through having been kept a day or two after it was made—the following appearances were observed:—The biconvex oval red corpuscles soon became globular, and in a very short time were observed to recoil or give a sudden jerk, a small portion of the contents being at the same time extruded in a direction opposite to that in which the recoil took place. In fact, the recoil seemed to be due to the sudden extrusion of a small portion of the contents of the corpuscle at one place, or in rare cases at two places, at the margin of the corpuscle. These small extrusions resemble, but are not identical with the "pullulations" described by Dr. Roberts as the effect of the action of a solution of tannic acid.*

It is very interesting to watch the sudden effect. The globular corpuscle suddenly jerks in one direction, and simultaneously with this, one observes a small mass of the contents of the corpuscle pouring out of a very fine aperture or crack at one side of the corpuscle. (Pl. I, fig. 1, *b*, *c*, *d*.) The motion or jerk seems to be caused by the sudden ejection of material, and so the corpuscle moves in an opposite direction. The opening is very small, and the extruded mass remains adherent to the corpuscle. It is much less than a twentieth the bulk of the corpuscle, and it is rarely larger than one-third the diameter of the corpuscle. It is slightly coloured and slightly granular, remains adherent to the corpuscle, and does not show any of the "hooded" character described by Dr. Roberts as the effect of tannic acid. The rent or crack in the envelope or at least in the now thickened rind of the corpuscle, through which the little mass is forced or ejected can often be clearly seen. If such a preparation be sealed up, such buds or projections may be kept for a considerable time. If a sufficient amount of the acid be added other effects follow. The corpuscles begin to assume a coarsely granular appearance, and the nucleus, until now unaffected, begins to assume a granular appearance, but still remains pale as at first. Owing to the granular condition of the hull (Dowdeswell) of the corpuscle, the nucleus cannot be seen until the centre of the corpuscle is focussed. Some of the corpuscles appeared to become granular without any extrusion of a part of their contents. In those corpuscles which had given way at one side the granular contents soon began to pass out through the rent, and often the nucleus also was extruded. In many of the corpuscles the nucleus was observed to be half within and half without the body of the corpuscle. A distinct, highly refractive envelope was traceable around the body of the corpuscle as far as the break in its side, when it suddenly ceased. This envelope was often traceable on one or both sides of the protruded part of the corpuscle for some distance, but never completely around it. The protruded mass generally remained attached to the side of

* "Proc. Roy. Soc.," 1863.

the corpuscle, even when the whole of its granular contents had passed out. The nucleus generally remained within the envelope, and became tinged of a yellow colour. The intranuclear plexus of fibrils was also revealed. (Pl. 1, fig 1, *g*.) The envelope of a corpuscle devoid of its granular contents was observed to be tinged of a yellow colour, and to show a double contour. A similar appearance was observed around the nuclei. The remaining corpuscles ultimately became completely disintegrated, and the field of the microscope became covered with free nuclei, granular colouring matter, and fragments of envelopes sometimes containing nuclei. (Pl. 1, fig. 1, *i*.) Sometimes an envelope was seen entire but with a rent in its side extending half through it, and through which its contents had escaped. (Fig. 1, *h*.) Sometimes a mass of granules was observed with a dark line partially enveloping it. The dark line represented a highly refractive envelope now distinctly shown—perhaps produced—by the action of the pyrogallic acid.

It is an interesting question to determine why and how this *sudden* protrusion of a portion of the coloured corpuscles is produced at one part of the circumference. That a process of endosmosis goes on is shown by the corpuscles assuming a globular form, but that of itself is not enough to account for the sudden protrusion already referred to, for water and many other fluids also produce endosmosis, but give rise to no such ejection of the contents of the corpuscles. The pyrogallic acid obviously has some chemical effect on the corpuscular contents, and it is just probable that the envelope of the corpuscle is not perfectly uniform in its resistance all round. We are inclined to view the above described phenomenon in connexion with the spots or thickenings described by Dr. Roberts as occurring in the wall of blood-corpuscles under the action of magenta solution, and also with the curious spots observed by us in the corpuscular wall after the action of gallic acid. It is to be noted that no contraction or diminution of bulk of the corpuscle is observable as coincident with the extrusion of the mass, which, however, is relatively so small that it would scarcely visibly affect the diameter of the corpuscle. The various effects are shown in Pl. 1, fig. 1.

Seeing that tannic acid has already yielded such remarkable results in the hands of Dr. Roberts, and that pyrogallic acid gave rise to equally peculiar phenomena, it occurred to us to ascertain the effect of a substance closely related to both, viz., gallic acid. [Wedel* also recognised that pyrogallic acid causes a separation of the hæmoglobin from the stroma.]

Gallic Acid.

A saturated solution of gallic acid causes the corpuscle to become

* Hermann's "Handbuch der Physiologie," vol. 4, p. 18, 1880.

spherical and the nucleus to become more distinct and tinged yellow. Then a sudden recoil or jerk is observed, the corpuscle at the same time elongating and swelling up. No rupture of the envelope was seen. The contents of the corpuscle appeared gradually to pass out, or to be dissolved out, and the field of the microscope became covered with granular *débris*. The nucleus was left of a bright yellow colour and quite smooth and homogeneous in appearance. As all the hæmoglobin is dissolved out of the corpuscle, the deeply stained homogeneous nucleus is seen in the interior of the globular corpuscle, which is surrounded by a highly refractive envelope, in which one, two, three, or more slight thickenings, or highly refractive elongated bodies, are to be seen. (Pl. 1, fig. 2, *b*, *c*.) Whether these bodies are at all comparable to the thickenings, or "maculæ," already described by Roberts, it would be difficult to say. Perhaps they are merely the *débris* of the intracellular stroma. Slight remains of this stroma may sometimes be seen in the perinuclear portion of the corpuscle, as after a time it becomes slightly tinged yellow and is stained by fuchsin or magenta. Within the nucleus one, two, or more highly refractive dots are always to be seen. When single the dot presents the appearance of a nucleolus, but it occupies very variable positions and is larger than the nucleolus revealed by the action of dilute alcohol, already described by Ranvier and one of us.* These dots are perhaps the remains of the intranuclear plexus. (Fig. 2.)

Hydrochloric Acid.

1 *per cent. Solution.*—One of us has already described the sudden enlargement and as sudden collapse of the corpuscles, with a simultaneous discharge of the hæmoglobin, which results from irrigation with a 1 per cent. solution of this acid.†

2 *per cent. Solution.*—On irrigating a drop of blood with a 2 per cent. solution of hydrochloric acid the nucleus began apparently to shrink away from its surroundings, becoming at the same time tinged of a yellow colour, and showing an intranuclear plexus of fibrils. In some of the corpuscles the nucleus had taken up an excentric situation, being placed at one end of the corpuscle or nearer one end than another, showing how plastic the perinuclear portion of the corpuscle is. (Fig. 3, *b*.) Its long axis was sometimes found to lie across that of the corpuscle. (Fig. 3, *h*.)

In some corpuscles the apparent shrinking of the nucleus was not observed and in some it was very slight. In a corpuscle seen on edge this change in the nucleus is very striking (fig. 3, *e*, *g*), and is perhaps due to the acid fluid passing by endosmosis through the body

* W. Stirling, "Journal of Anat. and Physiol.," vol. x, p. 778.

† W. Stirling, "Text-book of Practical Histology," 1882, p. 2.

of the corpuscle into the nucleus, and so affecting the latter as to cause it to shrivel up and thus to retreat from the mass of hæmoglobin in which it lies embedded.

In its passage through the body of the corpuscle the fluid had dissolved out a portion of the colouring matter of the corpuscle and carried it into the nucleus, so that the latter had become tinged with it. The nucleus generally retained its original position in the corpuscle with its long axis parallel with that of the corpuscle. Although apparently free in the hæmoglobin, the nucleus was not observed to change its position within its cavity. This may be accounted for on the supposition that some of the intranuclear fibrils pass through the envelope of the nucleus, and are in direct continuity with some of the fibrils which form the stroma of the hull of the corpuscle, and that these fibrils support the nucleus in the fluid by which it is surrounded. The size and coloration of the corpuscles were slightly diminished.

To appreciate the full effect of the acid it is necessary to have two views of the corpuscles, one on the flat and the other on edge. When viewed on the flat the shrunken nucleus can be seen lying within the corpuscle, with a clear wide space, probably filled with fluid, lying between it and the hæmoglobin-charged stroma. On causing the corpuscles to roll over, however, one observes that there is not only a shrinking of the nucleus, but also a simultaneous expansion of that portion of the corpuscle which lies immediately outside the nucleus, so that on edge the corpuscles, instead of presenting the usual graceful biconvex curves, suddenly bulge out in the centre, as represented in fig. 3, *f*.

A somewhat similar effect is described by Rollett* :—"The nucleus appears to be not very sharply defined, but frequently shrivelled and surrounded by an empty space, as though lying in a cavity of the substance of the blood-corpuscle (chromic acid, hydrochloric acid, nitric acid, picric acid, tannic acid, and concentrated tincture of iodine)." No figures are given, but we find most certainly that these acids yield more characteristic results than is conveyed in the above description. Corpuscles exhibiting this peculiar change can be kept for a considerable time. In some cases, just when the acid begins to act, a slight shrinking of the hæmoglobin from the envelope of the corpuscle can be seen.

It is curious to note the very different effects produced by solutions of the same acid—a 1 per cent. solution producing a *general* expansion of the whole corpuscle, whilst the stronger solution causes only a *partial* swelling up of one portion of the corpuscle, and a simultaneous shrinking of the nucleus.

* *Op. cit.*, p. 399.

Benzoic Acid.

This acid is soluble to the extent of 1 in 200 parts of water. A saturated solution causes the nuclei to become distinct and many of the corpuscles to become spherical. The contents of the stroma of the corpuscle gradually pass out through the membrane and the field of the microscope becomes covered with granular *débris*, which is in greatest abundance close to the corpuscles. The latter at length become quite clear, except for a few granules here and there in the stroma. The nuclei are distinct and of a bright yellow colour and are generally oval and more or less irregular in outline, but sometimes spherical. A double contour-line can nearly always be made out both around the nucleus and body of the corpuscle. Some of the nuclei show an intranuclear plexus, others are smooth and homogeneous.

Salicylic Acid.

A saturated watery solution of salicylic acid caused the corpuscles to swell up rapidly and become globular, the nuclei at the same time becoming tinged yellow.

In a very short time the corpuscles begin to elongate one after another, with a sudden jerk. No visible break could be observed in the side of the corpuscle, although the nucleus at the moment of recoil was often observed to pass out through the side of the corpuscle. The field of the microscope quickly became covered with yellowish *debris*—the contents of the stroma of the corpuscles—while the perinuclear part of the corpuscles became decolorised. Some of the nuclei were smooth in appearance, and bright yellow in colour; others showed beautifully the intranuclear plexus of fibrils, with narrow meshes; and still others had become swollen up to about twice their normal dimensions, and exhibited a wide-meshed plexus in their interior, due no doubt to the separation of the fibrils by the swelling up of the interfibrillar substance. Those nuclei which showed a plexus in their interior were observed to be bounded by a double contour-line, tinged of a yellow colour. A similar line indicated the position of the envelope of the corpuscle. Many free nuclei were observed, and some with the collapsed envelope and stroma of the corpuscle still clinging to them.

Other nuclei were noticed half within and half without the corpuscle. When the effect of the reagent had been more gradual, the perinuclear part of the corpuscle was granular and darkened in colour. The nucleus was pale—slightly tinged yellow—and showed an intranuclear plexus.

With this acid, as with many others, we frequently observed indications in the nuclei as if they were dividing, and we recall the observation of Preyer, that in the breeding season, he observed that

partially divided nuclei were frequently to be seen in the coloured blood-corpuscles of the frog.

Tartaric Acid.

On irrigation with a 12 per cent. solution of this acid, the first effect observed was an unequal shrinking of the corpuscles, so as to produce a very peculiar effect. Each corpuscle appeared with a series of bars across it, so as to present a series of alternate dark and light coloured areas. These areas not unfrequently resemble a series of folds or creases in the corpuscles. Sometimes these areas were arranged with considerable regularity across the corpuscle, whilst at other times they were irregular, and sometimes radiated outwards. The lighter areas seemed to be produced by the corpuscle becoming thinner at these parts, as if it were the result of osmosis taking place unequally and irregularly. This effect, however, soon gives place to another, wherein the corpuscles *suddenly* swell up and burst. Coincident with this swelling up, there is a great commotion in the material elements of the corpuscles, the nucleus is not unfrequently liberated, and can be seen to pass out of the disintegrated hull of the corpuscle, becoming at the same time completely decolorised. Immediately before bursting, the barred arrangement of the hæmoglobin just described disappears, and the nucleus, which until then had been pale and indistinct, becomes more distinct, yellowish in colour, and more granular in appearance. The swelling and decoloration could often be observed to commence at one end of the corpuscle and pass towards the other end. After the corpuscles had burst, the nuclei were left stained of a deep yellow colour, and showing a beautiful intranuclear plexus of fibrils. Many nuclei were seen with the collapsed envelope and stroma of the corpuscle still adherent to them. After a time the collapsed envelope and stroma often became slightly stained of a yellow colour.

Citric Acid.

The action of a 12 per cent. solution of citric acid was in every respect the same as that of tartaric acid.

Formic Acid.

On irrigation of a drop of blood with a 12 per cent. solution of this acid, the nuclei became distinct, and many of the corpuscles very soon became globular, gave way at one side, and became decolorised. The giving way was accompanied by a recoil or jerk. The nucleus sometimes escaped, but was generally surrounded by vestiges of the collapsed envelope, and of the stroma of the corpuscle. These effects were only seen in those corpuscles which first came under the influence of the reagent. In those corpuscles which were least exposed to the

reagent, the effect was more gradual, and somewhat different. The nucleus became brighter as with the other corpuscles, but after a short time the corpuscle suddenly expanded to several times its former size. No bursting was observed, nor did the corpuscle become globular before expansion. The nuclei became bright yellow and granular after the expansion of the corpuscle. The outline of the expanded hull, though faint, could be distinctly seen, as also could indications of a fibrillar stroma in the hull.

Lactic Acid.

A solution of lactic acid (24 per cent.) was found to cause at first an irregular crenation of the hæmoglobin *within* its envelope, so that the latter could be seen as a glass-clear structure, separated from its contents at different parts. The nucleus gradually became more distinct and "granular." The corpuscle soon expanded suddenly to several times its original size, the nucleus being well defined, and showing beautifully the fibrillar plexus in its interior. Some of the corpuscles doubtless burst, as free nuclei are found in many parts of the field. Nuclei are also seen with the remains of the hull and envelope of the corpuscle attached to one side.

Oxalic Acid.

A 2 per cent. solution of oxalic acid caused the corpuscles and their nuclei to swell up and become globular. The nuclei became tinged yellow. Very soon the corpuscles gave a sudden recoil or jerk, and at the same time elongated. The nucleus was often extruded at the moment of recoil, but in the case of other corpuscles it simply shifted its situation within the corpuscle. Not unfrequently we could watch the nucleus being extruded, and when it was half out and half in the corpuscle it was constricted, but still no envelope was visible in the corpuscle. No actual break was visible on the side of the corpuscle, but the contents of the latter was scattered over the field of the microscope in small yellowish granules. The corpuscles gradually lost their colouring matter, but the nuclei became stained of a bright yellow colour, and showed beautiful plexuses of fibrils in their interior. In many of the corpuscles also many fine fibrils, stained yellow, were observed in the perinuclear part of the corpuscles, mostly passing in a radial manner from the nucleus to the envelope. (Fig. 4, *e*, *h*.)

These fibrils and the nucleus are well stained by fuchsin or magenta.

If the action of the acid is gradual both nucleus and corpuscle may be completely decolorised.

Carbolic Acid.

The changes induced in the corpuscles by a saturated (1 in 20) watery solution of carbolic acid were peculiar, and somewhat difficult to describe on account of the rapidity with which the final stage was reached. The appearances seen were as follows:—On irrigating the blood with the solution of the reagent it was found that the corpuscles first affected had lost a great part—or the whole—of their hæmoglobin, while the nucleus had become much swollen and of a globular form. (Fig. 5, *a.*) The nucleus in this condition often showed well the plexus in its interior, but at other times it was seen to contain a number of dark yellow globules—derived no doubt from the perinuclear part of the corpuscle—but did not show the plexus. The corpuscle itself consisted of the swollen nucleus with only a narrow rim of the perinuclear part around it. In some corpuscles hæmoglobin was collected into a semi-globular mass attached to one side of the nucleus. (Fig. 5, *e*, *f.*)

In other corpuscles the nucleus had a crescent-shaped mass of hæmoglobin on either side of it. The hæmoglobin still in connexion with the nucleus, either in its interior or attached to its sides, was smooth in appearance and darkened in colour; over the part of the slide first invaded by the reagent many long dark streaks of granular colouring matter were seen. It appeared as if the corpuscles attacked by the acid had had their envelope dissolved or had burst at one side, allowing of the escape of the colouring matter.

On selecting some unaltered corpuscles in the centre of the slide, and watching the gradual action of the acid upon them as it passed under the cover-glass, the following appearances were observed:—The corpuscles seen on the flat first showed the barred arrangement of the perinuclear part already described as occurring under the action of several other reagents, and which is due to the varying thickness of the corpuscles at different parts. (This variation was seen in a corpuscle on edge, which has then a crenated appearance.) Very soon the corpuscles became granular in appearance, the hæmoglobin becoming at the same time much darkened. The nucleus became more distinct, was pale in colour, and had a granular appearance. Many of the corpuscles were observed to have given way at one side, and their contents to have been partly extruded. The nucleus soon became much swollen, and often exhibited an intranuclear plexus. The contents of the corpuscles were scattered about the field of the microscope in the form of small dark granules. Many of the darkly granular corpuscles above mentioned appear to become gradually decolorised, while in others the granules appeared to melt down into dark yellow homogeneous semi-fluid particles, which were seen adhering to the greatly distended nucleus.*

* The liberation of the hæmoglobin is of importance in connexion with poisoning

Ammonium Hydrate.

On irrigation of a drop of blood with a 12 per cent. solution of ammonium hydrate the corpuscles became spherical, and at the same time darkened in colour. The nuclei could not be seen distinctly, but appeared to have undergone the same change of shape as the corpuscles. Very soon the corpuscles *suddenly* collapsed, and they and their nuclei disappeared from view entirely. Sometimes, instead of collapsing, the corpuscles suddenly expanded, appearing to burst, and then melted away. The expansion was accompanied with a slight recoil or jerk. The solution of the corpuscles was sometimes more gradual, this depending, however, on the strength of the solution used. A 2 per cent. solution causes a more gradual solution of the corpuscles.

The effects of ammonium hydrate and the alkalies generally have been investigated very frequently, and solutions much weaker than we have employed have been used. Dr. William Addison* describes and figures what he called the acid and alkaline forms of human blood, while Kneuttinger† has shown that "alkalies as a general rule, when in a state of moderate concentration, exert a solvent action on all the constituents of the blood-corpuscles, including the nuclei." A solution of ·1 grm. in 100 cub. centims. is quite sufficient for this purpose. There is, therefore, nothing remarkable in a much stronger solution rapidly producing the same effect, but what we have found is that some time after complete solution of the corpuscles has occurred, small microscopic crystals are to be found scattered over the field of view. If such a preparation be sealed up the crystals gradually grow and assume a considerable, although still microscopic, size. In some cases these crystals are coloured of a slightly yellowish tint. Some are prismatic, whilst others are like two triangles placed with their obtuse angles towards each other. They resemble very closely in shape some of the forms of triple phosphate which are found in urine after decomposition of the urea has set in. At present we are unacquainted with their exact nature.

A very careful description of the action of the *vapour* of ammonia is given by Professor Lankester in his paper already cited, and it is curious to note that Professor Lankester saw particles separate from the hæmoglobin of frog's corpuscles, and "in these it was quite easy

by carbolic acid, when the urine has a dark smoky tint from the presence of altered hæmoglobin; Huls, under Landois' direction, also found that carbolic acid caused a separation of the hæmoglobin from the stroma. "Lehrbuch der Physiologie," 3rd edition, by Landois. January 10th, 1883.

* "Quart. Journ. of Mic. Sc.," N.S., vol. i, p. 20, and "Proc. Roy. Soc.," vol. 10, p. 186.

† "Zur Histologie des Blutes." Würzburg, 1865. (Stricker's "Histology," vol. i, p. 398.)

to recognize the well-known double rhomboid form of hæmoglobin crystals." The crystals which we found, however, were deposited in the fluid after the solution of the corpuscles under the action of ammonium hydrate.

Sulpho-Cyanide of Ammonium.

The action of this reagent on the coloured blood-corpuscle is, perhaps, one of the most interesting which we have examined—not only as regards its immediate action, but also as regards the ultimate effect it produces upon the nucleus in which it reveals an intranuclear plexus of fibrils with the greatest distinctness. (Fig. 8, *a*, *b*.)

On irrigating blood with a 10 per cent. solution of this salt, the corpuscles first became somewhat larger, and clear bands appeared in the perinuclear part. (Fig. 6, *a*.) The direction of these bands was generally across the long axis of the corpuscles. The corpuscle looked as if there were a series of folds or creases in it. The effect was thus similar to the early effect produced by citric and tartaric acids upon the corpuscles, except that the clear bands were more numerous in the case of the salt. The nucleus at the same time was brought out more distinctly, though still remaining pale, and the outline of its intranuclear plexus was faintly seen.

On selecting a corpuscle and observing the effect of the reagent closely, it was seen after a minute or two to lose its barred appearance, and then small, highly refractive, coloured globules began to form near the edge of the corpuscle. These small globules were soon seen on the outside of the corpuscle, to which they remained attached for a few seconds by a tailed process of their own substance. The droplets began to exhibit active molecular or Brownian movements. The processes connecting the globules to the outside of the corpuscle soon gave way, and then the spherical masses of coloured protoplasm began to dance about over the field of the microscope, in active molecular movement. (Fig. 6, *c*, *d*, *e*, *f*.) The margin of the corpuscle was left crenated, or rather dentated, and from the dentations other small globules began to come off and dance about like their predecessors. After some of these small globules were cast off, or at least after the threads which fixed them to the corpuscle were severed, which one can see taking place in the field of the microscope, the corpuscle often assumes a variety of shapes, giving out a lobose process, which may also change its shape and dimensions. (Fig. 6, *n*, *p*, *q*.) It is most interesting to watch the liberation of the droplets, and the variety of shapes assumed by the corpuscle. The detached droplets gradually become decolorised. The corpuscle begins to shrink visibly in size as the droplets extrude from it, and at the same time becomes darker in colour. The dentations disappear with the shrinking of the corpuscles, which commenced about five or six seconds after the first

globules were seen on the surface of the corpuscle. The outline of the corpuscle while shrinking was more or less irregular, and the droplets continued to form on the margins of the corpuscles. The corpuscles were obviously in a very *plastic* condition, if one may judge from the ease with which they changed their shape. Ultimately the corpuscle —or rather what remained of it—became condensed into a small globular mass of a dark yellow colour, usually with the pale nucleus in its centre. In a short time the nucleus which had hitherto been but slightly affected, suddenly expanded to a considerable extent, sometimes breaking up in the process.

A beautiful intranuclear plexus of fibrils was then seen to exist in the interior of the nucleus. (Fig. 8, *a*, *b*.) With the swelling up of the nucleus, the rest of the corpuscles underwent complete decolorisation. Traces of a stroma were detectable in the colourless hull of the corpuscle.

Ultimately the microscopic field contained a large number of nuclei, now considerably enlarged, and each one containing a beautiful view of its intranuclear plexus of fibrils. It was obvious that the nucleus had become enlarged through the swelling up of the material—whatever its nature—which lies within the meshes of the plexus. The fibrils themselves are also enlarged, and they bound meshes which in some cases are polygonal, in others hexagonal in shape. This reagent shows the intranuclear plexus quite as distinctly as ammonium chromate. On subsequently staining the distended nuclei with magenta or fuchsin, the plexus becomes stained, and they present a singularly fine demonstration of the arrangement of the fibrils. They may be kept for a considerable time.

One cannot fail to notice how closely the phenomena above described agree with the action of certain other reagents upon the blood-corpuscles—notably a 5 per cent. solution of ammonium chromate which shows the separation of particles of the hæmoglobin in the form of droplets of the most bizarre forms, and the changes of shape with the utmost distinctness. A strong solution of urea exerts an almost similar action upon the coloured corpuscles—and so does heat, as was described by Max Schulze.

Somewhat similar phenomena were observed by G. F. Dowdeswell,* in the blood of man and the dog when acted upon by septic matter, such as an aqueous extract of putrid muscle. These phenomena closely resemble the results obtained on human blood by Dr. Wm. Addison,† F.R.S., with pale sherry, either alone or in combination with various substances.

Coloured corpuscles of amphibian blood have been observed by Rindfleisch and Beale to undergo remarkable changes in shape.

* "Quart. Journ. of Mic. Sc.," 1881, p. 154.

† "Proc. Roy. Soc.," vol. 10, p. 186.

Rindfleisch is inclined to believe that small portions of the "hæmatin-containing contents must pass through pores in the cell membrane, or through holes produced in some other way in the same." But Preyer remarks* that such forms of coloured corpuscle have no membrane. Beale† found similar forms with long thread-like processes which became separated from the parent corpuscle when a drop of blood was warmed.

Sometimes another series of phenomena is obtained. Large globular or flask-shaped processes are given off from the margins of the corpuscles. (Fig. 6, *p*, *q*.) It may be at one side or on both. Some of them become detached, and coalesce to form globular or semi-globular large masses, which float off into the surrounding fluid, and are ultimately dissolved. Ultimately the nucleus undergoes the same changes as have already been described.

Sulpho-Cyanide of Potassium.

A 10 per cent. solution of this salt, gave exactly the same results as the corresponding salt of ammonium.

Ammonium Chromate.

When a drop of frog's blood was mixed in the cold with a drop of a 5 per cent. solution of this salt, the corpuscles were first observed to take on the barred appearance of the perinuclear part, described as part of the effect of citric, tartaric, and other acids, &c. The nucleus was more distinctly seen and was pale and slightly granular. Small coloured droplets were soon observed to form at the margin or periphery of the corpuscles. The corpuscles then usually became more or less regularly crenated, and the small yellow coloured droplets began to assume a beaded appearance on the surface or edges of the corpuscles. They then began to exhibit active Brownian movements. The corpuscles had by this time again become homogeneous in appearance, and appeared to be very mobile or plastic, as they changed their shape with great facility. The corpuscles diminished in size as the coloured droplets passed out, while at the same time the crenation disappeared, and the outline of the corpuscles became more or less uneven. The coloured part of the corpuscles often became aggregated into two or three or more rounded masses, causing a projection at those points. The corpuscle thus often had a dumb-bell, triradiate, or stellate form with rounded angles. Very often one or more of these knobs would become pedunculated, and at length break off from the corpuscle. Sometimes a corpuscle would be seen floating about with several of these coloured globular masses attached to it by long processes, while but a very small amount of colouring matter remained

* Preyer, "Virchow's Archiv," vol. xxx, p. 432.

† "Trans. Mic. Soc.," vol. xii, N.S., p. 32, and "Quart. Journ. Mic. Sc.," vol. iv, N.S., 1864.

around the nuclei. The processes attaching the masses of coloured matter to the nucleus often appeared to be membranous in character. Many of the corpuscles ultimately became perfectly spherical, the nucleus being indistinctly seen in the centre or at one side. These spherical corpuscles were often seen to contain several globular masses grouped usually around the nucleus. Very often the ultimate effect was to leave the nucleus with a dark yellow rounded knob of the coloured part of the corpuscle at either end of it. Sometimes the whole of the coloured part of the corpuscle had disappeared, leaving the nucleus pale and swollen, and with indications of a plexus in its interior. (These peculiar forms which the corpuscles assume may be preserved some time by sealing up the preparation.)

From some of the corpuscles long delicate processes were observed to pass. Some of these processes appeared to be made up of minute globules of coloured material which had coalesced to form a continuous bead-like string. They resembled very much the processes seen passing from the blood-corpuscles of the frog after treatment with a 20 per cent. solution of urea. Other processes appeared to be of a membranous character, and were tipped at their free extremities by a minute coloured globule.

All these processes were remarkable for their length, which was sometimes several times that of the corpuscle itself. They were more easily induced in the corpuscles by gently heating the slide over a spirit-lamp.

If some of the blood of a newt be kept for forty-eight hours in a 5 per cent. solution of ammonium chromate, it will be found on examining it that in most of the corpuscles the perinuclear part has entirely disappeared, leaving the nucleus much swollen and of a globular form. An intranuclear plexus with wide meshes is seen. The nucleus stains readily—though not very deeply—with picro-carmine, the interior of the nucleus becoming reddish in colour and the envelope yellowish. (Various forms assumed by the corpuscles are shown in Fig. 7.)

The action of this substance on the coloured blood-corpuscles of the frog is accurately described by Mr. Dowdeswell as far as regards the extension, retraction, and detaching of the protuberances, and he remarks that no rupture of a membrane, or anything of the kind, was to be seen, even with a power of 1,000 diameters. Dr. Klein* has also shown the immense importance of this substance for a variety of purposes, but especially for revealing the fibrillar plexuses within cells and nuclei, *e.g.*, in non-striped muscle, &c.

Urea.

A 20 per cent. solution of urea first caused the corpuscles to become

* "Quart. Journ. Mic. Sc.," 1878, 1879.

crenated and then to assume a globular form. This change to a globular form occurred for the most part suddenly with a recoil or jerk. At the same time processes were usually thrown out from the corpuscles, sometimes to a considerable distance. Usually the corpuscle itself underwent considerable changes in shape. The nucleus could not be seen. Small coloured droplets became extruded from the corpuscle and danced over the field of the microscope with active molecular movements. Sometimes the action of the reagent was less vigorous—due to less being present—and then the corpuscles gradually became globular, the sudden recoil not being observed. The crenation was observed as before, and then the corpuscle began to diminish in size, owing to the formation and detaching of its plastic coloured substance. All the corpuscles ultimately become spherical and very much lessened in bulk, and then gradually become decolorised. From some of those which had been of a globular form for some time long delicate beaded processes were observed to pass.

Kölliker* found that solutions of urea of 15 per cent. and upwards produced similar changes in frog's blood. "The coloured blood-corpuscles became gradually more jagged, and some became transformed into the most beautiful stellate cells with at most three to six tolerably long and more flask-like processes. The latter began as it were to dissolve and disappear, partly by their margins being dissolved, and partly by small coloured particles being detached from their surface. These particles at once became pale and gradually disappeared. At last there remained only the nucleus-containing portion of the cell, as a small, round, dark red, refractive globule, which eventually became pale, and which, even to the nucleus, disappeared without leaving a trace behind."

Preyer agrees that the above description is accurate, with the exception of the part which refers to their becoming pale, which Preyer ascribes to the action of water. Indeed, Preyer† obtained similar results by allowing a drop of solution of urea to evaporate on a slide and then placing a drop of blood upon the thin crystalline layer of urea thus formed. The results he obtained are carefully figured, and they agree exactly with the results we have obtained.

The interest which attaches to the solvent action of urea is considerable, but the remarkable variety of shapes and the detaching of droplets from the corpuscles are also interesting, more especially as urea is only one of a number of reagents which cause a similar reaction.

We propose to continue our observations upon the effects of the foregoing and other reagents upon human blood or mammalian blood generally, which will form Part II.

* V. Siebold u. Kölliker's "Zeitsch. f. Wiss. Zoolog.," vol. vii, 1855, p. 188.

† "Virchow's Archiv f. Path. Anat.," vol. xxx, p. 432.

DESCRIPTION OF PLATE 1.

Fig. 1. Effect of pyrogallic acid solution upon the red blood-corpuscles of the newt.
,, 2. Effect of gallic acid.
,, 3. Effect of hydrochloric acid.
,, 4. Effect of oxalic acid.
,, 5. Effect of carbolic acid.
,, 6. Various forms assumed by the corpuscles when acted upon by ammonium sulphocyanide or potassic sulphocyanide.
,, 7. Various forms produced by the action of ammonium chromate.
,, 8. Shows final effect of ammonium sulphocyanide on the nucleus, viz., to reveal an intranuclear plexus.

April 5, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a hitherto unobserved Resemblance between Carbonic Acid and Bisulphide of Carbon." By JOHN TYNDALL, F.R.S. Received March 15, 1883.

Chemists are ever on the alert to notice analogies and resemblances in the atomic structure of different bodies. They long ago indicated points of resemblance between bisulphide of carbon and carbonic acid. In the case of the latter we have one atom of carbon united to two of oxygen, in the case of the former one atom of carbon united to two of sulphur. Attempts have been made to push the analogy still further by the discovery of a compound of carbon and sulphur analogous to carbonic oxide, but hitherto, I believe, without success. I have now to note a resemblance of some interest to the physicist, and of a more subtle character than any hitherto observed.

When, by means of an electric current, a metal is volatilized and subjected to spectrum analysis, the "reversal" of the bright band of the incandescent vapour is commonly observed. This is known to be due to the absorption of the rays emitted by the hot vapour in the partially cooled envelope of its own substance which surrounds it. The effect is the same in kind as the absorption by cold carbonic acid of the heat emitted by a carbonic oxide flame. For most sources of

radiation carbonic acid is one of the most transparent of gases; for the radiation from the hot carbonic acid produced in the carbonic oxide flame, it is the most opaque of all.

Again, for all ordinary sources of radiant heat, bisulphide of carbon, both in the liquid and vaporous form, is one of the most diathermanous bodies known. I thought it worth while to try whether a body reputed to be analogous to carbonic acid, and, like it, so pervious to most kinds of heat, would show any change of deportment when presented to the radiation from hot carbonic acid. Does the analogy between the two substances extend to the vibrating periods of their atoms? If it does, then the bisulphide, like the carbonic acid, will abandon its usually transparent character, and play the part of an opaque body, when presented to the radiation from the carbonic oxide flame. This proves to be the case. Of the radiation from hydrogen, a thin layer of bisulphide transmits 90 per cent., absorbing only 10. For the radiation from carbonic acid, the same layer of bisulphide transmits only 25 per cent., 75 per cent. being absorbed. For this source of rays, indeed, the bisulphide transcends, as an absorbent, many substances which, for all other sources, far transcend *it.**

II. "On Electrical Motions in a Spherical Conductor." By HORACE LAMB, M.A., formerly Fellow of Trinity College, Cambridge, Professor of Mathematics in the University of Adelaide. Communicated by J. W. L. GLAISHER, M.A., F.R.S. Received March 14, 1883.

(Abstract.)

This paper treats of the motions of electricity produced in a spherical conductor by any electric or magnetic operations outside it. The investigation was undertaken some time ago in illustration of Maxwell's theory of electricity. This theory is so remarkable, more especially in the part which it assigns to dielectric media in the propagation of electro-magnetic effects, that it seemed worth while to attack some problem in which all the details of the electrical processes could be submitted to calculation, although it was evident

* Nearly twenty years ago I observed, among other changes of diathermic position, the reversal of bisulphide of carbon and chloroform, when the pale blue flame of a Bunsen burner was the source of heat. When, for example, the rays issued from a luminous jet of gas, the absorptions of the bisulphide and of chloroform were found to be 9·8 and 12 per cent. respectively; whereas when the Bunsen flame was employed, the absorptions of the same two substances were 11·1 and 6·2 per cent. The cause of this reversal doubtless is that in the Bunsen flame hot carbonic acid is the principal radiant. ("Phil. Trans.," 1864, p. 352.)—April 6.

beforehand from the researches of Helmholtz* and others that the results (so far as they are peculiar to the theory) would be of far too subtle a character to admit of comparison with experiment. In studying the mathematical character of the problems above stated I was led to a certain system of formulæ, which I have since utilised in two communications to the London Mathematical Society,† and which seem likely to be of use in a great variety of physical questions.

The first section consists mainly of a recital of the fundamental equations and of the conditions to be satisfied at the surface of a conductor. It is assumed, in the first instance, that the magnetic susceptibility of the conductor is zero.

In § 2 is introduced the assumption that all our functions vary as $e^{\lambda t}$, where t is the time, and λ a constant. It is pointed out that this assumption is sufficiently general. The fundamental equations are then put into a mathematically convenient form. Before, however, proceeding to apply these equations as they stand, I examine the effect of assuming that the velocity (v) of propagation of electro-magnetic effects in the medium surrounding the conductor is practically infinite. This assumption, which has been made by all writers (including Maxwell himself) who have applied Maxwell's theory to ordinary electro-magnetic phenomena, greatly simplifies the calculations without sensibly impairing the practical value of the results. If L stand for a linear dimension of the conductor and ρ for its specific resistance, it will appear in the sequel that when, as in all practical cases, λ is small as compared with v/L, the error introduced by the assumption in question is of the order $\lambda\rho/v^2$. For any ordinary metallic conductor, and for any value of λ which can be appreciated experimentally, this fraction is excessively minute.

In § 3 the solutions of our equations (on the assumption above indicated) are given in the form appropriate to our present problem. These solutions are of two distinct types. Those of the first type, which are much the more important from an experimental point of view, have (I find) been discussed, though by a different method, by Professor C. Niven, in a paper recently published.‡ As the points to which attention has been directed are for the most part sufficiently distinct in the two investigations, I have allowed the corresponding portions of my paper to stand.

In § 4 I discuss the case of electric currents started anyhow in the sphere, and left to themselves. The equation which gives the *moduli* of the natural modes of decay of the first type agrees with the result obtained by Professor Niven.

* Crelle, t. 72 (1870).

† "On the Oscillations of a Viscous Spheroid," "Proc. L. M. S.," Nov. 10, 1881, and "On the Vibrations of an Electric Sphere," May 11, 1882.

‡ "Phil. Trans.," 1882. The date of the paper is January, 1880.

In § 5 is studied the case of induced currents. Since any disturbance in the field (however arbitrary) can be expressed, as regards the time, by a series of simple harmonic terms, it is sufficient to consider the case when the variations in the inducing system follow the simple harmonic law. This case has, moreover, acquired a special interest since the invention of the telephone. The two extreme cases, where the period of the variation in the field is very large or very small in comparison with the time of decay of free currents in the sphere, are discussed in some detail.

In § 6 the case of a thin spherical *shell* is briefly examined.

I next proceed to investigate what modifications must be introduced into the methods and the results of the preceding sections when the substance of the sphere is susceptible of magnetisation. This occupies §§ 7, 8, 9, 10.

In the remaining sections of the paper I investigate the solution of our fundamental equations, taking account of the finite value of v. The corrections to our former results are of most interest in the solutions of the second type. Although the preceding theory, based on the assumption $v=\infty$, is sufficient for all purposes of comparison with experiment, there are certain processes of (at all events) theoretical interest of which it fails altogether to give an account, viz., all those cases in which any change in the superficial electrification of the sphere takes place. For the expression of these the solutions of the second type are appropriate. There is no difficulty in working out the requisite formulæ, but in the application to the case of *free* motion a difficulty of interpretation arises, which is noticed in the proper place.

III. "Observations on the Colouring-matters of the so-called Bile of Invertebrates, on those of the Bile of Vertebrates, and on some unusual Urine Pigments, &c." By CHARLES A. MACMUNN, B.A., M.D. Communicated by Dr. M. FOSTER, Sec. R.S. Received March 8, 1883.

(Abstract.)

In this paper the result of a systematic examination of the bile and various extracts of the livers of Mollusca and Arthropoda, and of the pyloric or radial cæca and other appendages of the digestive system of Echinodermata is described. The universal distribution of one colouring-matter, which by appropriate experiments is shown to be a chlorophyll pigment, is proved. It occurs in the above organs and can be detected in the bile of specimens of *Helix* after a six months' fast;

for this colouring-matter, since it is found in the appendages of the enteron, the name enterochlorophyll is proposed. The slight differences observable in different cases are shown to be due to the probable greater or less amount of the usual chlorophyll constituents,—blue chlorophyll, yellow chlorophyll, and chlorofucine,—and the presence of xanthophyll, lutein or tetronerythrin. Enterochlorophyll is shown to be much more abundant in the livers of Mollusca and in Echinodermata than in Crustacea, as the livers of the last generally contain more lutein, or sometimes tetronerythrin.

The presence of reduced hæmatin is also demonstrated in the bile of the crayfish and in several pulmonate Mollusca, and its respiratory and other uses discussed.

The conclusions which these observations and others led to are summed up as follows:—

(1.) The existence of enterochlorophyll in the so-called liver, or other appendages of the enteron in Invertebrates is definitely established.

(2.) This pigment occurs in greatest abundance in Mollusca, it occurs less frequently in Arthropoda, and its presence in Vermes is not proved.

(3.) The pyloric cæca of starfishes contain it in great abundance, also the intestinal appendages of *Echinus*, which fact shows that the former function like the so-called liver of other Invertebrates.

(4.) The bile of the crayfish and that of pulmonate Mollusca contains hæmochromogen; in the latter it is generally accompanied by enterochlorophyll, and appears to be concerned more in aerial than aquatic respiration.

(5.) The so-called liver of Invertebrates is a pigment-producing and storing organ, as well as being concerned in the preparation of a digestive ferment.

(6.) The presence of hæmochromogen in the bile of Invertebrates is apparently determined by their mode of living; and it does not appear to be distributed according to purely morphological considerations.

The remainder of the paper deals with vertebrate bile pigments, and contains some observations on abnormal urinary colouring-matters mainly with regard to their spectroscopy. The various bile pigments of Städeler are first dealt with, and some remarks on the bile spectra of animals follow; here it is shown that urobilin can be extracted from the liver of *Salamandra maculata* by means of alcohol, that it is absent from reptilian bile during hibernation, and that the liver of fishes may contain tetronerythrin which can be extracted from it by suitable solvents. The latter fact suggests an analogous function to that of the liver of Invertebrata.

The results of the examination of a green hydrocele liquid are

detailed, which showed beyond doubt that biliverdin was present, and since in that case its origin could be traced to blood pigment, the origin of biliverdin from blood pigment is demonstrated.

The identity of stercobilin and hydrobilirubin got by the action of nascent hydrogen on bilirubin is proved, and a difference between them and febrile urobilin shown to exist.

The statement that the absorption bands of sheep bile are the same as those which occur in Gmelin's reaction is shown to be erroneous, and a brief description of the method of isolating the colouring-matter of sheep bile and the wave-lengths of its different solutions given. Chlorophyll is shown to be absent.

Under the head of urinary pigments, it is shown that the feeble bands described by me in a former paper in the spectrum of febrile urobilin are not due to impurities, but are as much part of the spectrum as the band at F. Urohæmatin, and its difference from hæmatoporphyrin and its pathological significance are discussed. A simple method for the detection of indican in urine, some remarks on uroerythrin, on a peculiar red colouring-matter in pale urine, somewhat like urrhodin, follow. The deductions from this part of the paper cannot be very well given in the form of conclusions, and are therefore scattered throughout the paper.

A drawing of the microscopic structure of the liver of *Limax*, showing the enterochlorophyll within the liver cells, and maps of the most important absorption spectra described, accompany the paper. All readings are reduced to wave-lengths.

April 12, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Principal Cause of the Large Errors at present existing between the Positions of the Moon deduced from Hansen's Tables and Observation: and the Cause of an Apparent Increase in the Secular Acceleration in the Moon's Mean Motion required by Hansen's Tables, or of an Apparent Change in the Time of the Earth's Rotation." By E. J. STONE, F.R.S., *Director of the Radcliffe Observatory, Oxford.* Received April 3, 1883.

(Abstract.)

The errors in the Lunar Theory have been traced to the effects of changes in *the unit of time*, which have, apparently unconsciously, been introduced from time to time into astronomy, with changes in the adopted data.

The argument is clearly seen by a consideration of the different expressions for the longitudes of, what may be called, the mean sun, which have been adopted for the determination of the sidereal times at mean noon.

If B, H, and V denote the longitudes of the mean sun, according to Bessel, Hansen, and Le Verrier, we have for 1850, January 1, Paris mean noon, t.

$$B = 280^\circ\ 46'\ 36''\cdot12 + 1296027\cdot618184\ .\ t + 0\cdot0001221805\ .\ t^2.$$

$$H = 280^\circ\ 46'\ 43''\cdot20 + 1296027\cdot674055\ .\ t + 0\cdot0001106850\ .\ t^2.$$

$$V = 280^\circ\ 46'\ 43''\cdot51 + 1296027\cdot678400\ .\ t + 0\cdot0001107300\ .\ t^2.$$

In all these expressions the unit of time has been *supposed* to be a Julian year of 365·25 mean solar days.

The constant differences $7''\cdot08$ and $7''\cdot39$ in B−H and B−V are not unimportant, for they introduce abrupt changes in the record of time; but the differences in the coefficients of t and t^2 show that the *same* unit of time cannot have been adopted in these expressions.

The measure of time must be continuous, let, therefore, 1 and $(1+x)$ be the units in B and H,

$$\text{then } 1296027{\cdot}618184\,.\,t+0{\cdot}0001221805\,.\,t^2$$
$$=1296027{\cdot}674055\,.\,t(1+x)+0{\cdot}0001106850\,.\,t^2(1+x)^2.$$

If, therefore, $n=1296027{\cdot}674055$,

$$x=-\frac{0{\cdot}055871}{n}+\frac{0{\cdot}000114955\,.\,t}{n}.$$

To reconcile B and H, therefore, x must contain a variable term. Similar remarks apply to the difference between B and V.

Now, let N be the moon's mean motion referred to 1 as the unit of time, and $(N+\delta N)$ the moon's mean motion referred to $(1+x)$ as the unit of time,

then $$(N+\delta N)(1+x)=N,$$

and

$$t\,.\,\delta N=\frac{N}{n}\{0{\cdot}055871\,.\,t-0{\cdot}0000114955\,.\,t^2\}=0''{\cdot}747\,.\,t-1''{\cdot}54\left(\frac{t}{100}\right)^2.$$

But Hansen determined his mean motion of the moon so as to force an agreement between his theory and observations reduced with Bessel's unit 1; and his tables, therefore, represented the observations well for many years whilst 1 was adopted as the unit of time; but directly the unit of time was changed by the adoption either of H or V, then the effects of the erroneous determination of the moon's mean motion by Hansen became apparent. The change of error in longitude of Hansen's Lunar Tables between 1864, when Le Verrier's Solar Tables were adopted in the Nautical Almanac, and 1880 amounts to more than 10″.

The effect of the change of unit is also shown in the comparison of Le Verrier's Solar Tables with observation, but, of course, only to about the thirteenth part of the amount shown by the Lunar Tables.

The necessity of adopting some definite unit of time, by fixing the constants in the expression for the longitude of the mean sun, is insisted upon.

If $L_0+n_0t+s_0t^2$ is the expression adopted for the longitude of the mean sun, the quantities L_0, n_0, s_0 must never be changed. The correction δL, which from time to time may appear necessary to obtain the mean longitude of the sun from the longitude of the mean sun, must not be allowed to change the adopted values of L_0, n_0, and s_0. The true longitude of the sun will then

$$=L_0+n_0t+s_0t^2+\delta L+\text{Periodic terms.}$$

It would appear that speculations respecting changes in the time of

rotation of the earth on its axis are at least premature, until the theories have been revised with a unit of time freed from changes of adopted constants, which are at present inextricably mixed up with any effects which would result from a change in the time of rotation of the earth on its axis.

II. "On the Atomic Weight of Glucinum (Beryllium)." By T. S. Humpidge, Ph.D., B.Sc. Communicated by Professor Frankland, F.R.S. Received March 20, 1883.

(Abstract.)

In this paper the author shows that no conclusions with respect to the atomic weight of glucinum can be drawn from analogy of its compounds with those of other metals, and that this long-disputed question can only be decided by the specific heat of the metal or by the vapour-density of some of its volatile compounds. Two determinations of the specific heat have been made by Professor E. Reynolds and by M. Nilson, the former of whom obtained a result of about 0·6, and the latter only about 0·4. The probable inaccuracies in Professor Reynolds' apparatus are pointed out, and it is shown that his metal was probably impure.

The author has prepared metallic glucinum from the chloride, the vapour of which was passed over sodium contained in iron boats in a glass tube. A metal was thus obtained which had the composition:—

Gl	93·97
Gl_2O_3	4·71
Fe	1·32
Si	traces.
	100·00

and was probably the purest yet prepared.

The specific heat was determined by a modification of Regnault's method of mixtures, using electrical appliances to avoid the necessity of an assistant. Three determinations of the specific heat of silver in water, made to test the apparatus, gave the following results:—

I	0·05677
II	0·05568
III	0·05553
Mean	0·05600

and with a mean error of 1 per cent. The specific heat of metallic

glucinum was determined in turpentine, of which the specific heat was found to be 0·4231, and with the following results:—

I	0·4326
II	0·4264
III	0·4357
Mean..........	0·4316

and with a mean error of 0·8 per cent. Making a correction for the impurities contained in the metal, its true specific heat would be 0·4453, whence if the atomic weight is 13·65, the atomic heat becomes 6·08. This must, therefore, be the true atomic weight, and not two-thirds of this, or 9·1.

The number found by Nilson was somewhat lower than this (0·4079), and the above results may be slightly too high, firstly from hygroscopic moisture, and secondly from heat produced when the liquid was absorbed by the porous metal. About 0·66 gramme of the metal was used for the determinations, and it was compressed to a compact disk in a steel mortar.

The author is continuing the research.

III. "On a New Crinoid from the Southern Sea." By P. Herbert Carpenter, M.A., Assistant Master at Eton College. Communicated by W. B. Carpenter, C.B., M.D., F.R.S. Received March 15, 1883.

(Abstract.)

Among the collections of the late Sir Wyville Thomson, a small *Comatula* has recently been discovered which was dredged by the "Challenger" at a depth of 1,800 fathoms in the Southern Sea. Although it is unusually small, the diameter of the calyx being only 2 millims., the characters presented by this form are such as to render it by far the most remarkable among all the types of recent Crinoids, whether stalked or free. The name proposed for it is *Thaumatocrinus renovatus*.

It has only five arms, and in this respect resembles *Eudiocrinus*. But the basals, instead of becoming transformed into a rosette as in that genus, persist on the exterior of the calyx and form a closed ring of relatively large plates, which rest upon the centrodorsal. They support a ring of ten plates, five of which, alternating with the basals, bear the arms and are therefore the radials. These radials, however, do not meet one another laterally; for they alternate with five plates slightly smaller than themselves, which rest upon the

basals, and, with one exception, terminate in a free edge at the margin of the disk. The exception is the interradial of the anal side, which bears a short and tapering armlike appendage of five or six joints. It has no special relation to the anal tube, the lower part of which, like the peripheral portion of the disk, bears a pavement of anambulacral plates. But the centre of the disk is occupied by a relatively large and substantial oral pyramid, so that the disk in its general aspect resembles that of *Hyocrinus*.

Thaumatocrinus is thus distinguished by four striking peculiarities :—

(1.) The presence of a closed ring of basals upon the exterior of the calyx.

(2.) The persistence of the oral plates of the larva, as in *Hyocrinus* and *Rhizocrinus*.

(3.) The separation of the primary radials by interradials which rest on the basals.

(4.) The presence of an arm-like appendage on the interradial plate of the anal side.

Taking these in order—

(1.) No adult *Comatula*, except the recent *Atelecrinus* and some little known fossils, has a closed ring of basals; and even in *Atelecrinus* they are quite small and insignificant.

(2.) In all recent *Comatulæ*, in the *Pentacrinidæ* and in *Bathycrinus*, the oral plates of the larva become resorbed as maturity is approached. In *Thaumatocrinus*, however, they are retained, as in *Hyocrinus*, *Rhizocrinus*, and *Holopus*, representatives of three different families of Neocrinoids.

(3.) There is no Neocrinoid, either stalked or free, in which the primary radials remain permanently separated as they are in *Thaumatocrinus*, and for a short time after their first appearance in the larva of ordinary Crinoids. The only Palæocrinoids presenting this feature are certain of the *Rhodocrinidæ* (as understood by Wachsmuth and Springer), *e.g.*, *Reteocrinus*, *Rhodocrinus*, *Thylacocrinus*, &c. In the two latter, and in the other genera which have been grouped together with them into the section *Rhodocrinites* (W. and S.), there is a single interradial intervening between every two radials, and resting on a basal just as in *Thaumatocrinus*. But in the Lower Silurian *Reteocrinus* (of Billings; emend W. and S.) the interradial areas contain a large number of minute pieces of irregular form and arrangement.

(4.) It is only, however, in *Reteocrinus*, and in the allied genus *Xenocrinus*, Miller, which is also of Lower Silurian age,* that an anal appendage similar to that of *Thaumatocrinus* is to be met with.

* *Reteocrinus* occurs in the Trenton Limestone of Ottawa and in the Hudson River Group of Indiana and Ohio. *Xenocrinus* has as yet been found in the latter

Of the four distinguishing characters of *Thaumatocrinus*, therefore, one appears in one or perhaps in two genera of *Comatulæ*; another is not to be met with in any *Comatula*, though occurring in certain stalked Crinoids; while the two remaining characters are limited to one family of the Palæocrinoids, one of them being peculiar to one, or at most two genera, which are confined to the Lower Silurian rocks.

Their reappearance in such a specialized type as a recent *Comatula* is, therefore, all the more striking.

IV. "On the Structure and Functions of the Eyes of Arthropoda." By B. THOMPSON LOWNE, F.R.C.S., Lecturer on Physiology in the Middlesex Hospital Medical School, Examiner in Physiology in the Royal College of Surgeons, formerly Arris and Gale Lecturer on Anatomy and Physiology in the Royal College of Surgeons. Communicated by Professor FLOWER, F.R.S. Received March 30, 1883.

(Abstract.)

Three distinct forms of eye exist in the Arthropoda; the Compound eye, the Simple Ocellus, and the less known Compound Ocellus, common in larval insects, first described by Dr. Landois.

The relationship of the Compound eye to the Simple Ocellus is shown to be very distant, although I believe that these two types have been evolved from a common but very rudimentary primitive type. On the other hand, that between the Compound eye and the Compound Ocellus of a larval insect, is very close, the Compound eye being merely an aggregation of a great number of these ocelli, variously modified in the more highly differentiated Insects and Crustaceans. A fourth form of eye exists, in which the Ocelli are less closely united; this forms a connecting link between the compound eye and the compound ocellus. It is found in the Isopods, and may be conveniently termed the Aggregate eye.

The Simple Ocellus consists essentially of a pigmented capsule, behind a convex corneal lens, containing a cellular vitreous, which is separated from the retina by a fine fibrous membrane. The retina itself is a layer of Bacilla, comparable with those of Jacob's membrane in the Vertebrate, except that the highly refractive outer segments of the rods are turned towards and not away from the refractive media. The fibrous membrane, between the rods and the

group only. I cannot help suspecting that a better knowledge of this type will lead to its absorption into *Reteocrinus*.—P. H. C.

vitreous, is attached around its periphery to a structure which bears a strong resemblance to a ciliary muscle. This is enclosed in a ring-like sinus, which surrounds the ocellus almost as the canal of Petit runs round the lens of a Vertebrate. The vitreous is composed of a single layer of cuboid or prismatic cells, each with a nucleus near its inner extremity. These cells extend from the inner surface of the corneal lens to the outer surface of the fibrous membrane.

The Compound eye has a lenticular cornea beneath which the crystalline cones and great rods are placed. These are separated from the deeper nervous structures by a membrane comparable with the fibrous membrane of the Simple Ocellus; I have named this membrane the *Membrana Basilaris.*

The Membrana Basilaris is usually attached to the Cornea by an inflected ring of integument, the *Scleral Ring*, so that the Crystalline Cones and the Great Rods are entirely enclosed in a case. I have called all these structures the *Dioptron*, and have come to the conclusion that they are all Dioptric in function. They apparently correspond to the Cornea, Vitreous and Fibrous membrane of the Simple Ocellus.

The Membrana Basilaris, like the fibrous membrane, has a sinus around its periphery, and is connected with the inflected integumentary ring by fibres, which have a disposition similar to those of a ciliary muscle.

The Dioptron is nourished by Lymph Sinuses, which carry the circulating fluid from the Aorta* into the interior of the Dioptron and permit its exit into the common lymph spaces of the head.

Beneath the Dioptron is a nervous structure of great complexity; this I have named the Neuron.

The Neuron consists of a Retina, an Optic Nerve, and an Optic Ganglion.

The Retina is essentially a layer of rod-like bodies, Bacilla, supported by a delicate Neuroglia. The Bacilla are similar to the rods and cones of a Vertebrate in size, in form, and in structure, each has an outer highly refractive, and an inner protoplasmic segment. In some cases the outer segment is double, like that of the twin cones of fishes. In other specimens I have detected a *Lenticulus* between the segments. As in the Simple eye the highly refractive segments are turned towards the Dioptric media.

A layer of cells has been also demonstrated between the Basilar membrane and Bacilla; these in the majority of insects send pigmented fringes inward, between the outer segments of the Bacilla. The fringes are wanting in the diurnal flies, they represent the pigment layer of the Vertebrate retina.

I have spoken of the parts which underlie a single corneal

* I have so designated the anterior extremity of the dorsal vessel.

Lenticulus as a segment of the Dioptron. In many insects, especially in the larvæ, each segment has a distinct Retinula, consisting of a small bundle of Bacilla, which is connected with the ganglion by a distinct nerve enclosed in a separate pigmented sheath. I have named this form of retina *Segregate*. In other insects the retina is continuous over the inner surface of the Basilar membrane, but is connected with the deeper structures by a number of separate nerve bundles; whilst in the most highly developed Insects, a single decussating nerve connects a continuous retina with the ganglion. The ganglion consists of several nuclear and molecular layers, which are extremely like the corresponding layers of the retina of a Vertebrate.

All the structures of the Dioptron are developed from the cellular Hypoderm, whilst all the structures of the Neuron are formed from a solid papilla, or from a number of papillæ which are outgrowths from the Cephalic Ganglia, so that in this respect there is ground for a morphological comparison of the Dioptron with the dioptric structures, and of the Neuron with the nervous structures of the eye of a Vertebrate.

The Compound Ocellus of the larval insect is merely a single segment of a compound eye, with all the apparatus of the Dioptron and Neuron. I have used the term compound in relation to the refractive apparatus. The Neuron consists of a single bundle of Bacilla connected with the ganglion by a separate nerve bundle.

For several years I sought in vain for an explanation of the manner in which the compound eye could serve the purpose of vision. I discarded all the theories hitherto advanced, as being defective, and incapable of explaining the phenomena, consistently with the structure of the Great Rods.

Two years ago, whilst examining the recent eye of a small moth (*Pterophorus*), I was surprised to observe that the structure of the Great Rods was very different to anything with which I had previously been acquainted. The inner extremities of the Great Rods have been named Spindles, and are well known to present a very remarkable structure.

I first observed, in this moth, that the Spindles are, during life, large ovoid bodies, filled with transparent highly refractive fluid; the slightest injury gave rise to the escape of the fluid and left the Spindles in a shrivelled condition, the usual appearance of these bodies.

A further investigation has shown me that all compound eyes when uninjured have similar ovoid Spindles. These organs appear to act as magnifying and erecting lenses. Their anterior foci correspond to the position of the subcorneal images, and the posterior foci with the Bacillar layer of the retina.

It is well known that if an object-glass is placed in the reversed position beneath the stage of a microsoope, and the instrument is then focussed for its posterior focal plane, it can be used as a telescope. I regard the Dioptron as an aggregation of similar optical arrangements; the Corneal lens corresponds to the inverted objective, and the Spindle to the microscope.

I have made and given a series of measurements of the parts of an insect's eye in support of this view; the focal lengths of the corneal lenses, those of the spindles, and their relative distances, from each other, as well as the number and size of the corneal images are consistent with this theory. Therefore a continuous picture, *a mosaic of erect magnified central portions of the several subcorneal images*, falls upon the retina, and the sharpness of vision is not necessarily dependent on the number of corneal facets.

The complex modifications of the Dioptron appeared at first in many cases to offer insuperable objections to this view, and this necessitated a very careful reinvestigation of these structures, more especially in relation to the changes which they undergo in the preparation of sections for microscopic observation.

These researches have shown that many of the modifications observed are due to differences in the nature of the material of which the refractive elements are composed, not only in different genera and families, but even in the same species in different stages of development.

In many cases the refractive media consist of an oil-like fluid, which is decomposed or dissolved in the process of preparing the object for microscopic examination; in other cases the media consist in part at least of practically indestructible Chitin. And further the great elasticity of the parts gives rise to profound modifications the result of alterations of tension.

In the first, or introductory portion of my paper, I have reviewed the work of my predecessors with the object of showing the relation of previous observations to the theory which I have enunciated. The remainder of my communication is divided into four parts.

I. The Structure and Functions of the Dioptron.

II. The Structure and Functions of the Neuron.

III. The Development of the Compound Eye.

IV. The Morphology of the Eyes of Arthropods.

I. *The Structure and Functions of the Dioptron.*

In this portion of my paper the structure of the Dioptron is described, and its relation to the views I have adopted is discussed.

Perhaps the most important additions to our knowledge of this organ has been the discovery of the very important part played by the oil-like fluid already alluded to. This fluid is easily decomposed,

and resolved into a reddish granular precipitate and a transparent fluid, which mixes readily with water and saline solutions; it is blackened by osmic acid, and rapidly dissolved by ether, oil of cloves, and, though less rapidly, by alcohol.

A subcorneal lens has long been recognised in the eyes of Isopods, and has been regarded as a modified crystalline cone. Müller believed a similar lens to be present in the compound eyes of some insects. Of late this lens has been overlooked; I have, however, found that it really exists in the majority of Arthropods. It consists of the oil-like fluid just spoken of, enclosed in an elastic capsule. It gives the Cornea the peculiar brilliancy which it posseses during life. The fluid contents of the lens is permeated by a more or less dense stroma, which, when the oil is rapidly dissolved, by reagents, splits into four parts. These are the bodies described by Claparede as "*Semper's nuclei.*"

In some insects the lens can be isolated, and its capsule can then be ruptured by pressure on the thin cover-glass, so that the escape of the fluid can be actually observed. The empty capsules are then seen to be finely wrinkled, and usually torn by a single fissure.

In some insects the lens is developed from the cornea, in others from the outer portion of the crystalline cone.

The Spindles of the Great Rods also consist chiefly of the same refractive fluid, hence the profound modifications which they undergo when disturbed for purposes of investigation, or even as the result of *post mortem* change.

The formation of a subcorneal image as well as that of an erect image on the retina is discussed in this part of my paper, and the theory is shown by measurements to be in harmony with the actual conditions which have been observed.

The remainder of this part of my paper is occupied by a consideration of the principal modifications of the Dioptron.

I have recognised four distinct modifications of the Cornea, three of which exist in different stages of development in the cockroach. I have named these modifications,

I. Simple Continuous Cornea.

II. The Facetted Cornea.

III. The Kistoid Cornea.

The fourth modification is apparently confined, amongst insects, to the imago condition in the Gnats; in these the cornea consists of the crystalline cones of the nymph united to each other by a thin cuticular lamina. I have used the term lenticular to distinguish this form of cornea.

I have incorporated such knowledge as I have been able to glean with regard to the development of the cornea and subcorneal lens with this section of my paper.

The nature and modifications of the Crystalline Cone are next described; these afford an exceedingly difficult problem, on which further work will undoubtedly throw much light, especially in relation to the morphology of this organ. Some details with regard to the structure of the Great Rods are also added, which did not find a place in the general description of the Dioptron.

II. *The Anatomy and Functions of the Neuron.*

The Neuron consists of three parts—the Retina, the Optic Nerve, and the Optic Ganglion. The minute structure of these parts is fully described in this portion of my paper. The relation of the nerve fibres to the Bacilla and the Great Rods is also discussed. The optic ganglion consists of parts which are clearly comparable with the nuclear and molecular layers of the Vertebrate retina.

III. *The Development of the Compound Eye.*

The manner in which the Dioptron originates in the Hypoderm of the insect, as well as the nature and origin of the "Imaginal Disks," from which this structure is sometimes formed, is described. The development of the Neuron from the nerve-centres of the head presents features of extreme interest and importance, especially in relation to the phenomena of Ecdysis. The segregate retina of many larvæ is entirely replaced at the final Ecdysis by a newly formed retina, which is continuous, so that it appears as if a kind of internal Ecdysis affecting the epithelial elements of the nervous system occurs with the general integumental Ecdysis.

IV. *The Morphology of the Eyes of Arthropods.*

The final section of my paper is a short *résumé* of the Morphological relations of the different forms of Arthropod eye. These have been already alluded to in the commencement of this Abstract.

V. "Introductory Note on Communications to be presented on the Physiology of the Carbohydrates in the Animal System." By F. W. Pavy, M.D., F.R.S. Received April 5, 1883.

My last communication ("Proc. Roy. Soc.," vol. 32, p. 418) was entitled "A new Line of Research bearing on the Physiology of Sugar in the Animal System."

During the time which has since elapsed, I have been actively continuing my investigations in the direction started, and the results obtained give an entirely new aspect to the whole subject of the physiology of the carbohydrates in the animal system.

Modern research has shown that, besides the well-known carbohydrate principles such as sugar, &c., there are several dextrins distinguishable by their optical properties and their cupric oxide reducing power.

From the colloidal principle starch, which has no cupric oxide reducing power, principles (dextrins) are producible by the action of ferments possessing gradually increasing cupric oxide reducing power until maltose is reached, which constitutes the final product, and which possess a little more than half the cupric oxide reducing power of glucose.

This is one foundation point connected with the researches I have been conducting upon the physiology of the carbohydrates in the animal system.

The other foundation point is that the various members of the carbohydrate group are brought into glucose by the agency of sulphuric acid and heat.

Proceeding upon these facts, and taking the cupric oxide reducing power before and after subjection to the converting action of sulphuric acid and heat, I have prosecuted investigations upon the transformation of the carbohydrates within the animal system with the result of acquiring knowledge of an altogether unexpected nature.

Hitherto what has been observed as regards the transformation of carbohydrates by the action of ferments and chemical agents, has been a change attended with increased hydration—for example, the passage of starch into the successive forms of dextrin and maltose and cane-sugar into glucose.

The issue of the researches, however, which I have been conducting recently is to demonstrate the passage of carbohydrates exactly in the opposite direction by the action of certain ferments existing within the animal system.

Alike in the alimentary canal, the circulatory system and the liver, the conditions exist by which this kind of transformation is effected.

From the mucous membrane of the alimentary canal a ferment is obtainable which converts (1) glucose into a body possessing the same kind of cupric oxide reducing power as maltose; (2) cane-sugar into maltose, and not glucose as formerly asserted; and (3) starch either into maltose or a dextrin of low cupric oxide reducing power.

The presence of carbonate of soda modifies the action of a maltose-forming ferment, and leads to starch passing into a dextrin of low cupric oxide reducing power instead of into maltose.

The portal blood contains a ferment which possesses a maltose or a dextrin-producing power, and the contents of the portal system during digestion are charged with a notable amount of maltose sometimes, and at other times a low cupric oxide reducing dextrin.

After the introduction of glucose into the circulatory system, I have observed the presence of maltose.

The liver also contains a ferment capable, under certain conditions, of carrying glucose into maltose, and I have further witnessed, by the same kind of action as the sugars and dextrins are moved from one to the other, the conversion of a carbohydrate into the colloidal material belonging to the animal system (glycogen) which holds the analogous position of starch in the vegetable kingdom.

Evidence has likewise been supplied that by an action of the same nature as that which moves the carbohydrates from one to the other in the carbohydrate group, they are, under certain conditions, carried into a body out of the group, and thence not susceptible of being brought into glucose by the converting action of sulphuric acid; and, on the other hand, under other conditions, a substance is brought into the carbohydrate group, and its nature made recognisable by the converting action of sulphuric acid and its cupric oxide reducing power.

The subject as it even now presents itself is a large one, and I propose to deal with it in detail in a series of communications. The first will be devoted to that which refers to the alimentary canal.

April 19, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

I. "Measurements of the Wave-lengths of Rays of High Refrangibility in the Spectra of Elementary Substances." By W. N. HARTLEY, F.R.S.E., &c., Professor of Chemistry, Royal College of Science, Dublin, and W. E. ADENEY, F.C.S., Associate of the Royal College of Science. Communicated by Professor G. G. STOKES, Sec. R.S. Received March 20, 1883.

(Abstract.)

The authors describe a method of taking photographs of diffraction spectra produced by a small Rutherfurd speculum ruled with 17,460 lines to the inch. The lines in the spectra were accurately measured by the aid of a microscope magnifying 25 diameters and a dividing engine.

The length of the spectra which were taken on three different plates was 14 to 15 inches, and the measurements were accurate to the $\frac{1}{2000}$th of an inch. From these measurements the wave-lengths of the lines were calculated. The spectra include lines with wave-lengths 4674 and 2024. They were produced by electric sparks condensed by a pane of glass coated with tin-foil.

Of the electrodes used, *one always* consisted of cadmium, the other of the metal or the solution of the metal, or other elementary substance, the wave-lengths of the lines of which was to be determined; thus all the spectra were referable to the cadmium lines. Great accuracy is attainable by this method, and lines which have appeared identical or coincident in two different spectra, have thus been proved to differ in refrangibility.

All the spectra were compared with spectra obtained with the prism spectroscope described by one of the authors in the "Scientific Proceedings of the Royal Dublin Society," vol. iii, Part III, April, 1881.

Great care was exercised in taking the photographs, lest any irregularity in the surface of the plates should lead to inaccurate measurements. Gelatine films on specially selected *patent plate* glass

were used, and such a precaution is quite necessary. The photographs were *not* varnished. A certain number of lines measured by previous observers have been compared with the new measurements. Taking the numbers given by Thalén, Lecoq de Boisbaudran, and Cornu for 150 lines in the spectra of magnesium, zinc, cadmium, aluminium, indium, thallium, iron, &c., a close agreement with their measurements affords satisfactory evidence of the accuracy of these determinations. Besides the wave-length, a very careful description of the appearance of each line is given, together with its linear measurement indicating its position on a series of photographs obtained with the prism spectroscope, which series of photographs is presented with the paper. A distinction is drawn between those lines determined directly with the grating and others too faint to be seen on diffraction photographs, which were measured by the aid of the prism spectroscope and an interpolation curve $9\frac{1}{2}$ metres in length. The total number of lines measured and described is 2307, namely:—Magnesium, 42; zinc, 151; cadmium, 141; aluminium, 30; indium, 104; thallium, 70; copper, 164; silver, 124; mercury, 80; carbon, 20; tin, 129; lead, 86; tellurium, 322; arsenic, 112; antimony, 211; bismuth, 156; air, 215; and iron, 150.

A series of eighteen enlarged photographs, 36 inches in length, are presented with the paper, on which each line has its wave-length written over it.

II. "On the Limiting Thickness of Liquid Films." By A. W. REINOLD, M.A., Professor of Physics in the Royal Naval College, Greenwich, and A. W. RÜCKER, M.A., Professor of Physics in the Yorkshire College, Leeds. Received March 6, 1883.

(Abstract.)

The previous investigations of the authors have shown that the specific electrical resistance of a soap film thicker than 374×10^{-6} mm. is independent of the thickness, and that the composition of films formed of M. Plateau's "liquide glycérique" may be largely altered by the absorption or evaporation of aqueous vapour which attends even slight changes in the temperature or hygrometric state of the air ("Phil. Trans.," Part II, 1881, p. 447).

In the present paper they describe a modified form of the apparatus which they previously employed. The glass case in which the films are produced is surrounded by water, and additional precautions are adopted for maintaining the aqueous vapour within it at the tension proper to the liquid of which the films are formed. These changes

have entailed considerable alterations in details, but the main features of the apparatus remain unaltered. The new form, however, possesses the important advantage that the temperature and hygrometric state of the air in contact with the films can be kept perfectly constant during the progress of the experiments. With this apparatus a number of measures have been made of the electrical resistance of films which have thinned sufficiently to show the black of the first order of Newton's rings. The chief interest of these lies in the information which they afford as to the thickness of such films. To deduce the thickness from the resistance, it is necessary to assume that the specific resistance of the films is the same as that of the liquid in mass. The authors' previous experiments do not enable them to assert the truth of this assumption for such thin films, and it was therefore important to ascertain by an independent method whether it might be taken as approximately true.

For this purpose between 50 and 60 plane films were formed in a glass tube 400 mm. long, and 18 mm. in internal diameter. The tube was closed by pieces of plate glass, and placed in the path of one of the interfering rays in a Jamin's "interferential refractometer." When the films had become black, a known number were broken by bringing an electromagnet near to the tube, and thus moving some sewing needles, which had been enclosed along with the films. The mean thickness of the films was deduced from the displacement of the interference "fringes" caused by their rupture. For reasons given in the paper two tubes were used. One was placed in the path of each of the interfering rays, and the mean of the values obtained by breaking the films in each tube in turn was taken as the result of the experiment.

Two liquids were observed, viz., M. Plateau's "liquide glycérique," and a soap solution containing no glycerine. Details are given in the paper. The following are the means of the various groups of observations.

Liquid.	Method.	Mean thickness in terms of 10^{-6} millims.
"Liquide glycérique" ..	Electrical	11·9
	Optical	10·7
Soap solution	Electrical	11·7
	Optical	12·1

The agreement between these numbers is sufficiently close to make the fact that they are approximately correct unquestionable, and to prove that the mean thickness of a black film is nearly the same for both liquids.

The electrical observations afford a means of comparing the thicknesses of different black films, and observing whether the thickness of the black portion of any particular film alters as its area increases. The results obtained in the paper, and in a previous preliminary investigation on the same subject ("Proc. Roy. Soc.," 1877, No. 182, p. 334), are summed up by the authors as follows:—

(1.) Persistent soap films, which thin sufficiently to exhibit the black of the first order of Newton's rings, invariably display an apparent discontinuity in their thickness at the boundary of the black and coloured portions.

(2.) The whole of the black region at the time of, or very soon after, its formation, is of uniform thickness.

(3.) This thickness remains unaltered in any film, whether the coloured parts of the film are thinning or thickening, increasing or diminishing in extent.

(4.) It is different for different films, but no connexion has been traced between its magnitude and the time which elapses between the first formation of the film and the first appearance of the black, or between either of these and the time of observation.

(5.) The mean values of this thickness are the same to within a fraction of a millionth of a millimetre, whether the films are plane or cylindrical, in contact with metal or with glass, formed of soap solution alone, or with the addition of more than two-thirds of its volume of glycerine.

(6.) Two totally independent methods of measuring the thickness of the black portions of the films give concordant results.

(7.) The mean value of the thickness calculated by giving equal weight to the results of the electrical and optical experiments is $11 \cdot 6 \times 10^{-6}$ mm. The extreme values were $7 \cdot 2 \times 10^{-6}$ and $14 \cdot 5 \times 10^{-6}$ mm.

The smaller of these quantities is therefore a limiting thickness to which a soap film in air saturated with the vapour of the liquid from which it is formed rarely attains, and below which none of the films observed by us have thinned.

III. "On the Total Solar Eclipse of May 17, 1882." By ARTHUR SCHUSTER, Ph.D., F.R.S., and Captain W. DE W. ABNEY, R.E., F.R.S. Received April 9, 1883.

(Abstract.)

The first part of this paper gives an account of the journey and preparations for the eclipse. Three instruments were to be used during totality.

1. An ordinary camera with lens of 4-inch aperture, and focal length of 5 feet 3 inches.

2. A prismatic camera, that is, a camera with prism in front of the lens, or, in other words, a spectroscope without collimator. The refracting angle of the prism was 60°; its face 3 inches square. The camera had a corrected lens with a focal length of 20 inches in the yellow. The plate to be exposed was sensitive in the red as well as in the blue.

3. A photographic spectroscope, with one prism of a refracting angle of 62°, and a length of collimator and camera of 9 inches.

The general appearance of the corona seemed to the naked eye (of Dr. Schuster) not to have been strikingly different to that of the two previous eclipses, either in brilliancy or extension, but the photographs reveal very essential differences.

Some time observations were made at the first and last contact, and the length of the eclipse was measured to be 74 seconds.

The second part of the paper gives the results of a careful investigation of all the photographs obtained.

Three photographs of the corona itself with different times of exposure, viz., 3, 11, and 23 seconds, show a gradual increase in the extension of the corona. Care had been taken to fix, by means of a wire stretched across the camera, the position of the corona, and it is believed that the orientation is accurate within probably a quarter of a degree. The photographs show the prominences very well, and confirm the distinction which has been drawn between the inner and the outer corona. The shape of the corona was very irregular. A close connexion between the outline of the corona and the state of the sun's surface is placed beyond doubt. During the time of minimum sun-spots a great extension in a direction approximately coincident both with the ecliptic and with the sun's equator is observed, and we can generally trace a distinct line of symmetry nearly agreeing with the sun's axis of rotation. In addition to the long equatorial rifts, short but sharp rifts appear near the sun's poles. At times of great solar activity these rifts are not seen, nor is there any symmetry whatever in the general outline of the corona.

During the present eclipse the photographic impression of one of the rifts reached to a distance of 1·4 solar diameters away from the sun's limb. As regards form and general appearance of the streamers two points deserve special notice. One is the remarkable curvature of some of the coronal rays. The rays seem in many cases to start almost tangentially from the sun's limb; sometimes they are wider near the sun's limb, contracting as their distance increases; some of the rifts, however, spread out in a fan-like fashion. The second point to be noticed is the transparency of the streamers: in two instances at least we can trace structural details through the luminous streamers.

A drawing of the corona from the hands of Mr. W. M. Baillie shows a good agreement with the photographs.

The position of a comet which appeared during totality, can be accurately fixed by means of the photographs. At 18 h. 24 m. 36 s. G.M.T., the comet's place was found to be Dec. 18° 34′ 59″ N.; R.A. 3 h. 34 m. 43 s.

An examination of the different photographs shows a slight but progressive change in the comet's position. This is in part accounted for by the moon's motion over the solar disk during the eclipse; but part of it is very likely due to the proper motion of the comet, which apparently was receding from the sun during the eclipse.

Some interesting results were obtained by means of the prismatic camera. The strongest impression of the prominences was obtained in the ring corresponding to the calcium lines H and K. The hydrogen lines H_α (C) H_β (F) H_γ (near G) and H all appear in the strongest prominences; but differences are noticed in the relative intensity of some of these lines. Thus, one prominence is especially rich in violet light, and shows both H and H_α stronger than two adjacent prominences, which in their turn show a greater intensity of H_β. This can be explained on the supposition that the first mentioned prominence was hotter than the others, an explanation which is confirmed by the fact that it shows a great number of lines reaching far into the ultra-violet. The line ($\lambda = 5875$), which generally goes by the name of D_3, is also represented in the prominences, and a very weak impression of one prominence, corresponding to a wave-length 5315 (K 1474), can be seen. One of the prominences shows two lines in the infra-red; one of them corresponds very likely to $\lambda = 8240$, the other is beyond the limit of the normal spectrum published by one of us. Besides these well-defined prominences the photograph shows two rings, which are evidently due to the lower parts of the corona, and therefore correspond to true coronal light. The wave-length of one of these rings is 5315, the well-known corona line; the second ring corresponds to D_3. The yellow ring is much fainter than the green one, but more uniformly distributed round the surface of the sun.

An instantaneous photograph taken about five seconds after the end of totality shows still the prominences, and also at the cusps short extensions corresponding to the hydrogen lines, and due no doubt to the higher parts of the chromospheric layer.

The photograph taken with the spectroscopic camera shows close to the sun a strong continuous spectrum, reaching from F to a place beyond λ 3490 in the ultra-violet. At some distance away from the sun there is a sudden falling off in intensity, but traces of the continuous spectrum in the region near G can be seen up to a height of 1·47 solar radii on the southern side of the solar disk, and to a height of ·9 of a solar radius on the northern side.

A strong prominence, which was cut by the slit, gives a complicated spectrum. The calcium lines, and especially the lines H and K, stand out prominently. Then, as might be expected, all the hydrogen lines are represented, including those in the ultra-violet, photographed by Dr. Huggins in star spectra. Some unknown lines bring up the total number of lines photographed to 29. In the outer regions of the corona the continuous spectrum is traversed by the reversal of the solar line G and by a number of faint lines. About thirty of these coronal lines have been measured.

In conclusion, we may briefly review the results we have obtained. The direct photographs of the corona are chiefly of interest in connexion with previous and future eclipses, and we believe that those we have obtained will be found of value, as they have been taken during a time of maximum sun-spots, as they extend further than any photographs previously obtained, and as the position of the corona in the sky has been fixed by means of them to within a fraction of a degree.

The photograph taken with the prismatic camera is of importance when we come to compare spectra of different prominences, which are found to give lines with different relative intensities caused no doubt by differences of temperature. Two prominence lines in the ultra-red have been discovered. It is also proved that the green line of the corona is a line specially belonging to the corona. It is only very faintly present in the prominences, but forms a distinct ring round a large part of the solar disk. A faint ring corresponding to D_3 is also seen.

The photograph of the spectrum of the corona and prominences has yielded an abundant harvest. Twenty-nine lines of one prominence have been photographed, and the great importance which the metal calcium plays in the solar eruptions has been emphasized. Other lines well known hitherto as chromospheric lines, but not traced in the prominences, are now shown to belong to them also, and a number of unknown lines, especially in the ultra-violet, has been added to the list.

As regards the corona we may point out that only one line has hitherto been well determined, and accepted as a true corona line, though one or two more have been suspected. During the late eclipse the corona seems to have been especially rich in lines. Thollon observed some in the violet without being able to fix their position; and Tacchini could determine the position of four true corona lines in the red. We have been able to photograph and measure about thirty additional lines.

The fact that part of the outer corona shines by reflected light has been once more proved by the presence of the dark Fraunhofer set of lines G, and if any doubt previously existed respecting the presence

of dark lines in the coronal spectrum, that doubt is now completely removed.

The results have amply proved the value of the photographic method employed, and it has been shown how an eclipse of only seventy seconds' duration can be made to yield important information.

IV. Note on *Syringammina*, a New Type of Arenaceous Rhizopoda." By Henry B. Brady, F.R.S. Received April 10, 1883.

[Plates 2, 3.]

The specimens to which the following note refers were dredged in the Faroë Channel in the autumn of last year, during the cruise of H.M.S. "Triton," and were sent to me for examination by Mr. John Murray, F.R.S.E., under whose direction the scientific observations of the expedition were carried out.

It is now a well-known fact that the region lying between the north coast of Scotland and the Faroë Islands possesses certain features of unusual interest owing to the existence, side by side, of two sharply defined areas, of which the bottom temperature differs to the extent of 16° or 17° Fahr. The depth of the two areas is very similar, ranging from 450 to 640 fathoms, and they are separated by a narrow ridge having an average depth of about 250 fathoms. The physical aspects of this phenomenon have been the subject of much discussion, and the biological conditions attendant thereupon are of almost equal importance; indeed, so far as the Rhizopoda are concerned, there are few areas of the same extent that have so well repaid the labour of investigation. On the "Lightning" Expedition of 1868, superintended by Dr. Carpenter and Sir Wyville Thomson, the cold area furnished amongst other interesting organisms, the large Lituoline Foraminifer *Reophax sabulosa*, a form which has since been obtained near the same point on the cruise of the "Knight Errant," but has never been met with elsewhere. The warm area yielded at the same time *Astrorhiza arenaria*, a large sandy species previously unknown to British naturalists. On the "Porcupine" Expedition of 1869, another modification of the latter genus, *Astrorhiza crassatina*, was obtained in the cold area; and near the boundary line an entirely new arenaceous type was dredged, to which the generic named *Botellina* has been assigned by Dr. Carpenter. From the fact that all the specimens of the form appeared more or less broken, it has been inferred that the tests were adherent when living; but the fragments were abundant, and consisted of stout tubes, many of them upwards of an inch in length, the interior being subdivided by a labyrinth of irregular

sandy partitions. More recently, in 1880, on the cruise of the "Knight Errant,"* the rare genus *Storthosphæra* was found in the warm region, and in the cold area specimens of *Cornuspira* which measured more than an inch in diameter, rivalling in size the finest of the tropical Orbitolites, and therefore amongst the largest known Porcellanous Foraminifera.

The bottom-dredgings obtained on the cruise of the "Triton" in August and September, 1882, have not been fully examined, but the surface-gatherings made by means of the tow-net are remarkable for the abundance of the curious pelagic type *Hastigerina.* This genus had not previously been found living in the British seas, and the specimens procured were equal in size and beauty to any of those collected in southern latitudes during the "Challenger" voyage.

Of the Rhizopoda contained in the dredgings, by far the most noteworthy is the arenaceous form which I propose to describe in the present paper. It may be stated at the outset that two specimens were secured, but owing to the excessively fragile nature of the organism, both were in a more or less fragmentary condition, though sufficient remains to indicate their principal structural features.

The general appearance of one of the specimens, drawn to the natural size, is shown in Pl. 2, figs. 1, 2, 3; the second was too much broken to be of service except for purposes of dissection. The figured specimen is about an inch and a half (38 millims.) in diameter, and about eight-tenths of an inch (20 millims.) in thickness, but it is probable that the latter dimension may not be much more than half that of the entire organism; indeed, it is evident that the test when complete was a rounded mass, which if developed with any degree of symmetry, must have been a sphere of about an inch and a half diameter. The structure revealed by the fractured surfaces is that of a congeries of branching and inosculating tubes radiating from a common centre.

The fragile nature of the investment is due to the fact that the walls are composed of fine sand with scarcely a trace of inorganic cement. In this respect the organism bears a close resemblance to several well-known arenaceous Rhizopods, notably to *Astrorhiza arenaria*, but the difference in size renders the absence of incorporating cement a much more noticeable feature; for whilst the test of the latter species, though loosely arenaceous, has sufficient strength and substance to bear handling without injury, that of the present form will scarcely support its own weight when taken out of water, and crumbles into a mass of sand on the gentlest attempts at manipulation. It is hardly possible to lift even small fragments by means of forceps, and the specimen would have been in less satisfactory

* "Proc. Roy. Soc. Edinb.," 1882, vol. xi, pp. 708–717.

condition than they are, were it not that the disintegrated portions formed a layer of sand in the bottom of the bottle, partially embedding the larger pieces. Owing to this want of cohesion it has been found impossible to prepare thin sections of any part of the test.

The inferior aspect of the specimen, represented in Pl. 2, fig. 1, is entirely a fractured surface, and is probably something approaching a median section; but it is much too uneven to show any regularity of structure, except at some points near the periphery. The only portion remaining of what was originally the exterior of the test is shown in the side view, fig. 3, at the point marked *a*. The convex or "superior" aspect of the specimen, as it stands on the plate, exhibits chiefly the open ends of the transversly-broken tubes.

The different portions of the structure examined in detail reveal little beyond what may be realised at the first glance.

The "inferior" surface of the specimen displays somewhat more regularity in the radial arrangement of the tubes than could be made apparent in the drawing, owing to the unevenness of the fracture. The organic centre appears to have been broken away, and it is impossible to say whether there has been originally any true nucleus, in the shape of a well-defined primordial chamber. The central portions, so far as they are left, consist of a network of branching and often contorted tubes, of somewhat smaller diameter than those of the exterior, and less regularly disposed (Pl. 3, fig. 8).

Nearer the periphery the system of tubes takes a distinctly radial character, and in a favourable section appears divided into concentric layers or tiers of gradually increasing depth (fig. 6). The concentric "partitions" exhibited in the radial section of the test, fig. 6, *d.d.*, are not, like the "labyrinthic layers" of *Parkeria*, continuous septa of cancellated structure, but are formed by lateral branches, given off at intervals, which unite so as to produce a more or less regular network (fig. 7). As nearly as can be made out, there may have been ten or eleven such reticulated "partitions," at intervals varying from $\frac{1}{20}$ inch (1·26 millims.) near the centre, to $\frac{1}{10}$ inch (2·5 millims.) near the periphery.

As already stated the tubes are not of uniform diameter, those near the centre measuring sometimes no more than $\frac{1}{50}$ inch (0·5 millim.), whilst near the exterior they often exceed $\frac{1}{25}$ inch (1 millim.), the average diameter being about $\frac{1}{35}$ inch (0·735 millim.). The external surface is granular, but in the dry condition it is tolerably smooth; the interior is smooth and well finished. The internal cavity whether of the radial tubes or the branches is continuous, exhibiting neither constrictions, septa, nor labyrinthic subdivision. The thickness of the walls is about $\frac{1}{200}$ inch (0·125 millim.).

The peripheral ends of the tubes are rounded, and closed by an

aggregation of sand-grains of somewhat lighter colour than the rest of the test, in precisely the same way as in *Astrorhiza arenaria* and its immediate allies. The rounded terminations are shown in the side view, fig. 3, at the point marked *a;* and on a larger scale in fig. 5.

With regard to the animal inhabiting the test, there is not much to be said. When examined by Mr. Murray, fresh from the dredge, the tubes were partially filled with dark-coloured sarcode; and in the preserved specimens, the peripheral portions of the fragments that have been dissected were in this condition. Owing to the intermixture of sand-grains it has been found impossible to examine the tube-contents under high magnifying powers, but they appear in all respects similar to the sarcode found in the tests of many of the larger arenaceous Foraminifera which have been preserved in the same way, namely, a dark, somewhat firm, granular, gelatinous mass, which on drying forms nearly black branching threads.

There can be no doubt that the organism described in the foregoing paragraphs is the representative of a new type of arenaceous Rhizopoda, and the generic term *Syringammina* (σῦριγξ, ιγγος, a pipe, ἄμμος, sand) with the trivial name, *fragilissima*, appears appropriate for its designation. In the absence of complete specimens its zoological characters cannot be fully stated, but the following will servé for its identification.

Syringammina fragilissima, nov. gen. et sp.

Test free; consisting of a rounded mass of branching, inosculating tubes radiating from a common centre, and arranged in more or less distinct concentric tiers or layers, which are marked by the formation at intervals of a network of lateral branches. Walls arenaceous, composed of nearly uniform fine sand, with little or no inorganic cement. Apertures terminal, situated at the peripheral ends of the tubes, closed in with loosely aggregated sand-grains. Colour dark grey when wet, drying to a much lighter tint. Diameter about $1\frac{1}{2}$ inch.

The precise habitat of the specimens is given in the following note from the log of the "Triton:"

"Station 11. August 28th, 1882,—lat. 59° 39′ 30″ N., long. 7° 13′ W.; depth 555 fathoms; ooze. Surface temperature, 57°·2; bottom temperature 45°·5 Fahr."

The position is to the west of the Wyville Thomson Ridge, and close to the "Holtenia Ground" of the "Porcupine" Expedition. Mr. Murray informs me by letter that "the dredge employed on this occasion was of very much lighter description than those generally used in deep-sea dredging. It came up with a large quantity of ooze in the bag, the top layers of which were of pale brown colour, soft and watery, the deeper layers somewhat compact and of slaty hue.

One of the specimens rolled out of the oozy layer of the deposit when the dredge was emptied on the deck and broke, unfortunately, in the hands of the sailor who lifted it; the other was found on passing the mud through the sieves, and when first observed appeared quite spherical."

I learn that a somewhat similar specimen was dredged at a depth of 1000 fathoms off the Azores, during the "Challenger" cruise, but that it went to pieces in the sieve.*

A few words must be added respecting the zoological position and affinities of the new genus. On the whole, *Syringammina* finds its nearest allies, so far as living Foraminifera are concerned, in the deep-sea varieties of *Astrorhiza*. Comparing it with *Astrorhiza arenaria*,† its investing walls are found to be constructed in precisely the same way of loosely aggregated sand, and even in the size of the grains there is great similarity, though this may be in a measure accidental. But whereas the test of *Astrorhiza* consists (typically) of a few tubes, generally unbranched, radiating on one plane from a central cavity or chamber, that of *Syringammina* is formed of a multitude of tubes which radiate nearly equally in all directions, and have numerous branches which inosculate freely. In *Astrorhiza*, as in *Syringammina*, the peripheral ends of the tubes serve as the general aperture; and in both the orifices are masked by aggregations of loose sand, forming rounded and apparently closed terminations.

The genus *Parkeria* has already been referred to in describing the mode of increase by concentric layers, and both in size and general contour there is considerable resemblance between *Syringammina* and the fossil type. But the similarity of internal structure, apparent on a comparison of some of the drawings now furnished, with the illustrations accompanying the original memoir on *Parkeria* and *Loftusia*,‡ is much more remarkable and cannot be passed over without notice. Owing to the difference in the magnifying powers employed, the resemblance in the drawings is more striking than in

* It may be of service to those who have the opportunity of dredging, to note that the sandy skeletons of organisms of this sort may be sufficiently strengthened to bear handling by placing the specimens for a time in strong alcohol, and then drying; afterwards, when thoroughly dry, saturating with a very dilute solution of dammar in benzole, and draining on blotting-paper. The dammar solution should be so weak that it does not leave a gloss on the surface of the specimen when finished.

† M. Sars, Carpenter, and Norman assign these deep-sea sandy forms to the same genus as the shallow-water organism, *Astrorhiza limicola*, which has a chitinous investment, coated with soft mud. I have not disturbed the arrangement, but my impression is that they represent two distinct genera.

‡ "Phil. Trans.," 1869. Compare for example the structure of *Syringammina* as shown in figs. 6, 7 of the present paper with that of *Parkeria* and *Loftusia* as represented in some of the figures in Plates 73 and 79 of the memoir referred to.

the specimens; nevertheless the radiate tubular structure and the concentric arrangement of the parts are features common to both forms. On the other hand, the cancellated layers of *Parkeria*, which form continuous septa of greater or less thickness, are only represented in *Syringammina* by an open network of anastomosing tubes. Mr. Murray has called my attention to the close similarity that exists between the texture of the natural surface of the recent form, and that presented by some infiltrated specimens of *Parkeria*, after being etched by means of acid.

Morphologically, however, *Syringammina* appears to find a closer parallel in the group of fossil Rhizopods described by Professor Duncan under the term *Syringosphæridæ*.* Of these the test in its typical condition is a spheroidal body from 1 to 3 inches in diameter, composed of radiating tubes open at their peripheral ends. The tubes, which are branched and inosculating, are arranged in conical bundles radiating from the centre of the test, and the intervening spaces are filled with an accessory network of branching tubes which present a variety of characters. The walls are formed of granular carbonate of lime. The tubes of this fossil type are of much smaller diameter than those of *Syringammina*, and their association in conical bundles is a very distinctive feature; besides which, the test presents no evidence of concentric structure.

The material at present available for investigation is insufficient for any detailed comparison of the structure of these organisms, but it is amply sufficient to show that there exist analogies of great interest between the groups they respectively typify; and it encourages the hope that living specimens may yet be found that shall satisfactorily elucidate the still doubtful points in the organization of the fossil types.

EXPLANATION OF THE PLATES.

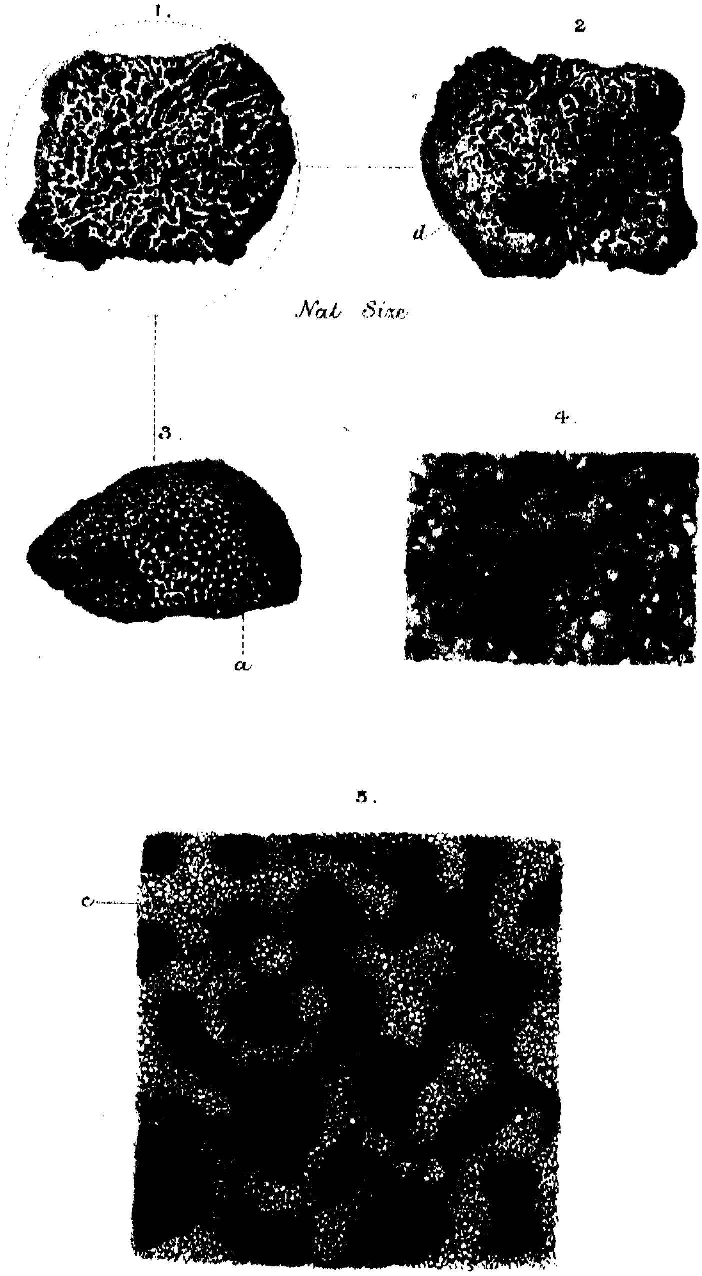
Plate 2.

Figs. 1, 2, 3. *Syringammina fragilissima*, natural size.

1. Inferior aspect, representing an uneven fractured surface near the middle of the specimen. The dotted line indicates approximately the original outline.
2. Superior aspect of the specimen, representing chiefly an uneven fractured surface near the periphery. At *b* the exterior is coated with a film of dried sarcode.
3. Lateral aspect. The portion marked *a* represents the uninjured natural surface.

* "Karakoram Stones or Syringosphæridæ," by Professor P. Martin Duncan, M.B., F.R.S., &c., in the "Report on the Scientific Results of the Second Yarkand Mission," 4to, 3 plates. Calcutta, 1879.

Also "On the Genus *Stoliczkaria*, Duncan, and its Distinctness from *Parkeria*, Carpenter," "Quart. Journ. Geol. Soc.," 1882, vol. xxxviii, p. 69, Pl. 2.

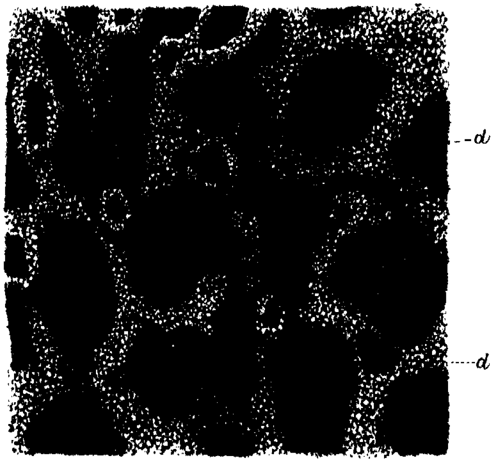


A. T. Hollick, ad nat. del. et lith.

Mintern Bros. imp.

SYRINGAMMINA FRAGILISSIMA.

6.



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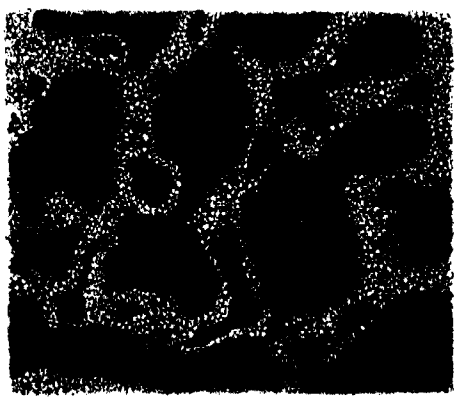


8.

A. T. Hollick, ad nat. del et lith. Mintern Bros imp.

SYRINGAMMINA FRAGILISSIMA

Fig. 4. Section of the test, magnified 50 diameters.
The section is of very unequal thickness, but serves to show the arenaceous structure of the test, and the character of the constituent grains, many of which are minute Foraminifera.

Fig. 5. A portion of the surface at *a* (fig. 3) magnified 8 diameters; showing the closed terminations of the tubes; and, at *c*, a portion of one of the concentric reticulated "partitions."

PLATE 3.

Fig. 6. Radial section (fractured surface) magnified 8 diameters; *d.d.* reticulated "partitions."

Fig. 7. Tangential section (fractured surface) on the plane of one of the reticulated "partitions" (*d.d.*), magnified 8 diameters.

Fig. 8. Inferior aspect; a portion magnified 8 diameters, showing the smaller size and contorted form of the tubes near the centre of the test.

April 26, 1883.

THE TREASURER, V.P., in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Contributions to the Chemistry of Food." By JAMES BELL, Ph.D., F.C.S. Communicated by Professor FRANKLAND, F.R.S. Received April 4, 1883.

(Abstract.)

This paper contains the results of researches on butter, cheese, milk, the cereal foods, bread and lentil flour.

The author some time ago, as the result of a series of experiments, indicated that it was probable the soluble and insoluble fatty acids in butter fat did not exist as simple glycerides, but in the complex form of compound ethers—palmitic and oleic acids being combined in the same molecule with butyric acid. The results of a further investigation into the character of butter fat are given, which tend to confirm this theory of its constitution. Butter fat is proved to vary in composition far beyond the limits previously supposed, and a table of representative samples is given, showing the ordinary variations which occur. Ordinary fats are contrasted with butter fat, and it is sug-

gested that the latter, from its complex character, probably performs some more specific office in the system than the former.

The proximate analyses of ten descriptions of cheese are given, and the composition of the fat extracted has been determined in each case. The soluble and insoluble fatty acids are shown to possess the normal relation existing between these acids in milk fat, a result held to be inconsistent with the views advanced by some chemists that the albuminoids become slowly changed into fat.

Tabular results are given of a wide and comprehensive investigation into the variations which occur in the composition of the milk yielded by different cows under the varying conditions of food and season. Besides cow's milk, the proximate constituents of other kinds of milk have been determined, and as the analyses of the whole of the milks have been conducted on an uniform method, the results will be found valuable for purposes of comparison.

The changes which occur in sour milk have been investigated and the results given, with a statement of the amount of depreciation which occurs in the non-fatty solids, according to the period for which the milk has been kept.

Tabular results are given of the proximate analyses of the different cereals, of wheat flour, and of oatmeal, and also a complete analysis of the ash of each. The proximate constituents of the cereals, &c., have been partly determined on new lines, and partly by an improved method of analysis.

Judging from the variable results obtained by different chemists, the author suggests that the saccharine matter appears in some instances to have been overlooked, while in others it must have been determined in an aqueous extract of the cereals, without regard to the transformations which the soluble albuminoids produce in starch and other carbohydrates in presence of water.

The albuminoids of the cereals have been found to possess varying degrees of diastatic action in converting starch, rye standing at the top, and rice at the bottom of the scale.

Tabular results of the proximate analyses of aërated and home-made bread are given; the changes which occur in flour during the baking process have been studied, and the sugar present identified as maltose. The results of a proximate analysis of lentil flour made on the same lines as the cereals, are given, and also a complete analysis of the ash of lentils.

II. "Pelvic Characters of *Thylacoleo carnifex*." By Professor OWEN, C.B., F.R.S., Director of the Natural History Department, British Museum. Received April 13, 1883.

(Abstract.)

In this paper the author selects from a series of fossils transmitted from Australia since the communication of the 1st February, 1883, the pelvis of a mature *Thylacoleo*, and gives results of a comparison of it with that of *Macropus major*, *Felis Leo*, and *Dasyurus ursinus*, incidentally referring to pelvic characters of the Wombat, the Koala, and the Phalangers.

The results are that the few correspondences with the Kangaroos relate exclusively to a common marsupial nature; to these, in the Dasyurines, are added other resemblances not found, save in Carnivorous Marsupials; and, finally, prominent characters are shown in which *Thylacoleo* exclusively repeats those presented by the pelvis of *Felis Leo*.

III. "On the Continuity of the Protoplasm through the Walls of Vegetable Cells." By WALTER GARDINER, B.A., late Scholar of Clare College, Cambridge. Communicated by W. T. THISELTON-DYER, C.M.G., F.R.S. Received April 16, 1883.

(Abstract.)

After quoting a passage from Professor Sachs' "Vorlesungen über Pflanzen-Physiologie," "every plant however highly organised is fundamentally a protoplasmic body forming a connected whole, which, as it grows on, is externally clothed by a cell membrane and internally traversed by innumerable transverse and longitudinal walls," the author suggests that any observations which demonstrate an actual continuity in organs of large extent must be of interest, as tending to show the truth of Sachs' statement in a sense somewhat more literal than his own. At the time that the above remarks were written, the instances of the existence of any protoplasmic continuity between adjacent cells were but few, being limited to sieve tubes and to Tangl's results with regard to the endosperm cells of Strychnos, Phœnix, and Areca. Then came the author's investigations upon the pulvini of Mimosa, Robinia, and Amicia, and subsequently to them, but previous to the present communication, appeared an important paper by Russow, in which he had proved that in the bast parenchyma cells and the phloem ray cells of numerous plants, *e.g.*, Populus,

Salix, &c., the closing membranes of the pits were perforated by fine protoplasmic threads. In the present paper the author details his results upon pulvini, treats of the methods employed, and gives an account of his investigations as to the structure of endosperm cells, which were undertaken with the object of controlling his previous researches. Since experiments showed that all preservative reagents were unsatisfactory, fresh material alone was employed. In investigating the subject of protoplasmic continuity the method of swelling the cell-wall and subsequent staining was adopted. Either sulphuric acid or chlor. zinc. iod. was used as the swelling agent; and, after washing, the sections were stained with Hoffmann's violet—in which case they were subsequently washed out with glycerine—or with Hoffmann's blue. The latter dye was found to be a particularly satisfactory reagent for staining the protoplasm alone, while methylene blue, on the other hand, especially stains the cell-wall. After the action of chlor. zinc. iod. and subsequent staining it was still found to be impossible to colour the protoplasmic threads running through the cell-wall, and since the author's experiments had led him to believe that this was merely due to phenomena of diffusion (the solution of the colloidal dyes diffusing but little into the colloidal protoplasm), he adopted the modification of dissolving the solid Hoffmann's blue in a 50 per cent. solution of alcohol saturated with picric acid, which was found to be perfectly successful as a stain.

Having shown that in its reactions the pit membrane differs markedly from the rest of the cell-wall, the author proceeds to give a detailed account of his results with pulvini. In Mimosa, Robinia, and Amicia, the parenchymatous cells of the pulvini were found to communicate with one another by means of delicate protoplasmic threads which perforated the closing membranes of the pits. In many instances it appeared as if the thread went bodily through the pits, but the author was disposed to believe that in reality a sieve-plate arrangement was present in every case. The protoplasm of the bast fibres also appear to communicate through the pit membrane by means of a sieve-plate-like structure. Thus from the epidermal cells right up to the last living bast fibre, which impinges on the first dead vessel, a direct continuity from cell to cell has been established, and such a pulvinus may be regarded as a connected whole. The author has observed that a means of communication between adjacent cells appears to exist in the pulvini of Phaseolus multiflorus, and Desmodium gyrans; in the cells of the leaf of Dionæa muscipula; in the stamens of Cynara Scolymus, and in tendrils; but in consequence of somewhat hurried observation, owing to the lateness of the season, he cannot regard these results as entirely conclusive, and intends to work over the subject in further detail on a future occasion.

In order to clear up certain doubtful points with regard to his work

on pulvini, and to set his investigtions on the firmest possible basis, the author now commenced the study of endosperm cells, since in them the cells were exceptionally large, and the pit membrane being very thick, the presence of any threads running through its substance would be likely to be clearly seen. Having confirmed Tangl's results with Strychnos, Phœnix, and Areca, he examined in detail the seeds of some fifty species of palms, and besides those of typical representatives of the following orders:—Leguminoseæ, Rubiaceæ, Myrsineæ, Loganiceæ, Hydrophyllaceæ, Iridaceæ, Amaryllidaceæ, Dioscoriaceæ, Melanthaceæ, Liliaceæ, Smilacæ, and Phytelephasieæ—in all of which he found that the cells were placed in communication with one another by means of delicate threads passing through the walls of the cells. In unpitted cells, *e.g.*, Tamus and Dioscorea, the threads traversed the whole thickness of the wall. In the greater number of instances the cells were pitted, and the threads passed across the pit membrane; and in certain cases, *e.g.*, Bentinckia, Kentia, Howea, Lodoicea, and Asperula, communication was established both through the thickened walls and through the pits. The endosperm cells displayed in their structure every possible modification, both of thickness or thinness of the pit membrane, of clearness or difficulty of observation, and of degree of development of the middle lamella. The development of the endosperm was not worked out in any case, but the cells were shown to communicate with one another at a very early period. When sections of living endosperm tissue were treated with sulphuric acid, and stained with Hoffmann's blue, the same results were obtained as with pulvini, only here everything was on a much larger scale. Thus both the methods and results received every confirmation.

The author then treats of his investigations on the subject of Plasmolysis, in which he had established that when the plasmolytic condition is induced in a cell the contracted primordial utricle does not lie free in the cell-cavity, but is connected on every side to the cell-wall by means of numerous fine protoplasmic threads. His experiments lead him to the conclusion that the above phenomena do not give any definite assistance or confirmation to the study of perforation of the cell-wall, for as often as not the threads bear no relation to the pit, the only significance implied being that the protoplasm and the cell-wall are intimately connected the one with the other.

Finally, the author remarks that, although he is aware of the danger of rushing to conclusions, yet that when his results, which were foreshadowed by Sachs and Hanstein when they demonstrated the perforation of the sieve-plate, are taken in connexion with those of Russow, it appears extremely probable that the communication between adjacent cells not only takes place in the parenchymatous cells of pulvini, in the phloem parenchyma cells, in the cells of

endosperms, and in the prosenchymatous bast fibres, but is of much wider if not of universal occurrence. At any rate we were now in a position to get a clearer insight into such phenomena as the downward movement of a sensitive leaf upon stimulation, of the wonderful action of a germinating embryo on the endosperm cells, even on those which are most remote from it, of the action of a tendril towards its support, and of a series of phenomena in connexion with general cell mechanism, which were too numerous to mention, and could not be treated of in his present paper.

The paper is accompanied by forty figures, which illustrate the principal instances of protoplasmic continuity referred to in the text.

IV. "On the Dependence of Radiation on Temperature." By Sir William Siemens, F.R.S., D.C.L., LL.D. Received April 25, 1883.

Sir Isaac Newton held that the radiation of heat from a hot body increased in arithmetical ratio with the difference of temperature between it and the surrounding bodies. This law forms a rough approximation to the truth over a very limited range of temperature. MM. Dulong and Petit carried out an elaborate experimental research on the rate of cooling of hot bodies by radiation, extending to somewhat higher temperatures, and deduced from their observations the empirical formula—

$$\text{Rate of cooling} = m(1{\cdot}0077)^{t}(1{\cdot}0077^{T-t}-1).$$

Here T is the temperature of the hot body in degrees Centigrade, t the temperature of the surrounding matter, and m is a constant depending on the nature of the radiating body. This formula agrees very fairly with experimental results for ordinary temperatures, but, like Newton's law, it has been shown that it cannot be applied for a wider range.

The anomalous results which Newton's law and the formula of MM. Dulong and Petit lead to, when applied to the cooling of bodies at a very high temperature, are well illustrated by the attempts at deducing therefrom the temperature of the solar photosphere. Waterston and Père Secchi (in his work entitled "Le Soleil"), following Newton's hypothesis, obtained 10,000,000° C. as the probable solar temperature, and Captain J. Ericsson, on the same hypothesis but assuming other constants, arrived at a temperature between 2,000,000° and 4,000,000° C. Strangely contrasting with these determinations are those of Pouillet in 1836, and Vicaire in 1872,

who, employing Dulong and Petit's empirical formula, deduce the values 1461° and 1398° C. for the solar temperature. Between these extreme estimates we have those of Dr. Spoerer, 27,000° C., of Zoellner, 27,700°, Professor James Dewar (1872), 16,000°, Rosetti (1878), 9000°, and Hirn (1882), 20,000°.

In my own investigations on this subject, by comparing the spectrum of the sun as regards the proportion of luminous rays with those of the electric arc and gas flames, I have arrived at the conclusion that the temperature of the photosphere does not exceed 2800° C., which is in close agreement with the limit assigned by M. Sainte-Claire Deville, deduced from the observations of Frankland and Lockyer on the hydrogen lines in the solar spectrum. Sir William Thomson, in a paper communicated to the Philosophical Society of Glasgow (1882), has compared the power of the sun's radiation per unit of surface with that of a Swan incandescent carbon filament, and has shown that it is about sixty-seven times greater; he concludes from these data that the estimate I had formed of the solar temperature, *i.e.*, nearly 3000° C., cannot be very far from the true value.

These diverse and indirect results have long impressed me with the need of further experimental investigation of the dependence of radiation on temperature; and it has occurred to me lately, that the difficulties with which Dulong and Petit had to contend in making their measurements by means of a mercurial thermometer, where the losses due to conduction and convection are very great, and exceedingly difficult to determine, might be avoided in adopting a method of conducting the experiment which forms the principal subject of my present communication.

It is well known that the measurement of electrical currents and resistance is susceptible of very great accuracy compared with all thermal measurements; hence my endeavour has been to estimate thermal effects entirely by electrical methods. In the Bakerian Lecture for 1871, which I had the honour of delivering before the Royal Society ("Proc. Roy. Soc.," vol. 19, p. 443), I showed that the resistance of a platinum wire can be expressed as a linear function of its temperature by an empirical formula, the constants of which must be determined for each individual wire; hence conversely, if resistance of a wire previously calibrated is measured, its temperature can be deduced. From theoretical considerations I showed that

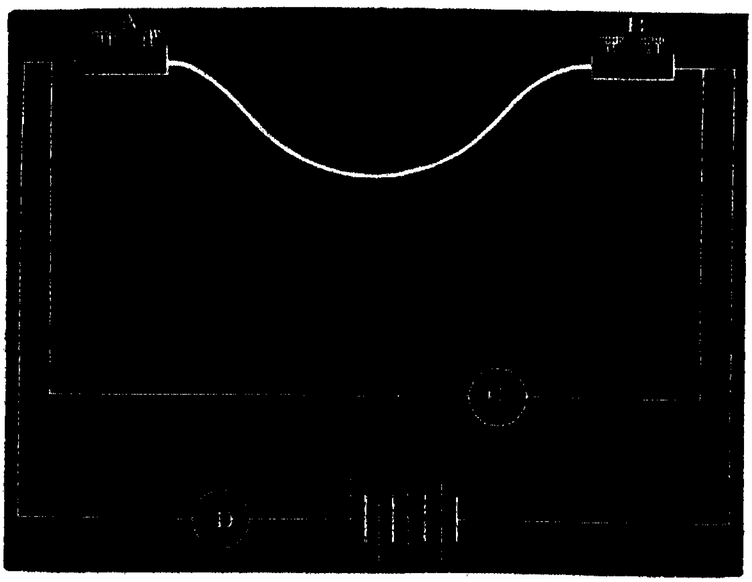
$$\frac{r}{r_0}=\alpha T^{\frac{1}{2}}+\beta T+\gamma$$

might be expected to represent the relation between the resistance and absolute temperature. This formula agreed closely with my own experimental results for platinum, copper, silver, iron, and aluminium wires ("Journal of the Society of Telegraph Engineers and Elec-

tricians," vol. i, p. 123, and vol. iii, p. 297), and has since been verified by Professor A. Weinhold in the case of platinum from 100° to 1000° C. ("Annalen der Physik und Chemie," 1873, p. 225).

The apparatus which I propose for determining the dependence of radiation on temperature consists of a platinum or other wire, 0·76 millim. in diameter, suspended between two binding screws, marked (A) and (B) on the diagram, carried on two suitable wooden stands. The binding screws are connected through an electro-dynamometer (D), for the purpose of measuring the current, to a secondary battery,

Diagram showing arrangement of experiment.



the number of cells in which can be varied. A high resistance galvanometer (G) is also inserted between the binding screws as a shunt to the platinum wire.

The electro-dynamometer is of the ordinary form, in which the current passes through a fixed coil, and a movable coil consisting of a single twist, hung by a torsion spring in a vertical plane at right angles to the plane of the fixed coil. The couple due to the current is balanced by the torsion of the spring, hence the angle of torsion is proportional to the square of the current. The current through the high resistance galvanometer being a measure of the difference of potential between the extremities of the platinum wire, the reading of the galvanometer, divided by the main current as determined by

the electro-dynamometer, is proportional to the resistance of the wire. Hence the constant of the instrument and the resistance of the galvanometer being known, the resistance of the platinum wire could be calculated, as the current was varied by altering the number of cells composing the battery.

The measurements were made in all cases when equilibrium had been established between the radiation and the energy of the current, as evinced by the constancy of the readings of the electro-dynamometer and galvanometer.

Having made a rough preliminary series of experiments to test the suitability of the method and apparatus, with satisfactory results, on April 17th I made a second series, the results of which are recorded in Table I. Column I gives the current in ampères passing through the wire; column II the difference of potential in volts between the terminals as deduced from the readings of the galvanometer; column III the rate at which the energy of the current was converted into radiant energy, represented by the product of the electromotive force and current, and therefore measured in volt-ampères or watts; column IV the resistance of the wire, being the ratio of the electromotive force to the current; column V the corresponding temperature of the wire in degrees Centigrade. Finally, column VI describes the condition of the wire as apparent to the eye.

Table I.

Length of wire 102 centims. Diameter 0·76 millim.

Temperature of room 65° F.

I.	II.	III.	IV.	V.	VI.
Ampères.	Volts.	Watts.	Ohms.		
2·91	1·192	3·468	·4096	—	Just warm to touch.
3·999	1·639	6·555	·4099	—	
5·738	2·831	16·24	·4933	100°	
8·943	5·662	50·64	·6331	282	
12·27	9·536	117·00	·7772	570	Chars wood.
16·66	16·39	273·0	·9838	881	Very dark red.
13·19	11·175	147·4	·8472	653	Red heat.
20·90	22·052	460·9	1·055	1075	Bright red.
23·73	26·82	636·4	1·130	1194	Very bright.

On April 18th, three further series of experiments were made, the results of which are set forth in a similar manner in Tables II, III, and IV.

Table II.

Length of wire 102 centims. Diameter 0·76 millim.

Current increasing.

Temperature of the room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding temperature of wire.	
63·5° F.	2·565	·895	2·295	·3489	—	Just warm.
,,	3·217	1·340	4·310	·4165	—	
,,	6·36	3·204	20·377	·5037	120°	Hot.
,,	8·511	5·146	43·798	·6046	250	
,,	10·714	7·599	81·416	·7029	420	Chars cotton.
66·0	13·192	11·026	145·45	·8358	645	Discolouring.
,,	13·698	11·927	163·38	·8707	690	Dark red.
,,	15·595	14·602	227·72	·9363	816	Light red.
67·0	16·222	15·510	251·60	·9561	852	Bright red.
,,	17·869	19·072	340·02	1·0698	960	Yellow.
,,	25·094	29·80	747·86	1·1875	1260	White.

NOTE.—The temperatures corresponding to the very small currents are not given, as for very small deflections the electro-dynamometer readings could not be regarded as perfectly trustworthy.

Table III.

Length of wire 102 centims. Diameter 0·76 millim.

Current increasing.

Temp. of the room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding temperature of wire.	
60° F.	2·744	·908	2·491	·3309	—	Just warm.
,,	3·629	1·483	5·382	·4086	—	
,,	6·79	3·278	22·258	·4827	125°	Hot.
,,	8·995	5·364	48·251	·5963	270	Nearly chars cotton.
,,	11·072	7·465	82·653	·6742	430	Chars cotton.
,,	14·048	11·925	167·52	·8489	700	Dark red.
70°	16·247	15·496	251·76	·9538	855	Light red.
,,	19·299	19·97	385·40	1·0348	1005	Bright red.
,,	20·073	20·577	413·04	1·0251	1037	Very bright red.
,,	22·948	25·643	588·45	1·1175	1164	Yellow.
,,	23·634	26·25	620·40	1·1107	1185	Bright yellow.
,,	25·171	28·31	712·59	1·1247	1240	White.
,,	26·190	29·80	780·46	1·1379	1272	,,

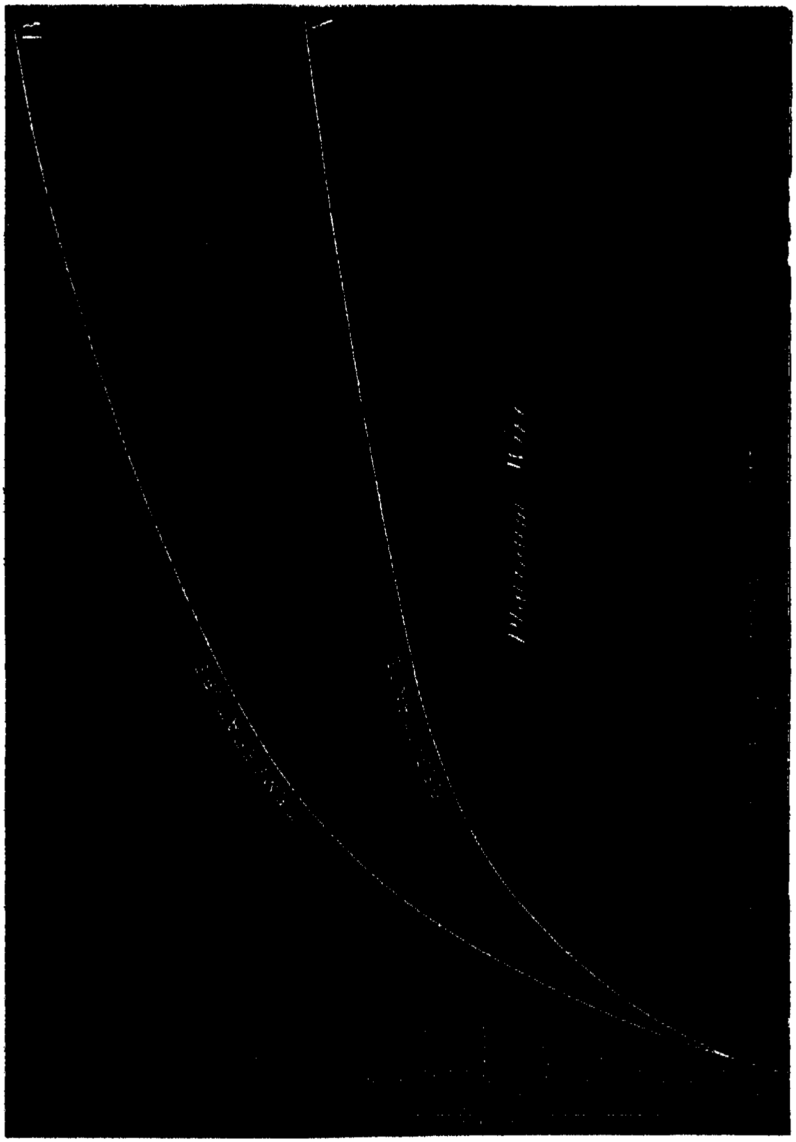


Table IV.

Wire the same as in III.

Current decreasing.

Temperature of the room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding temperature of wire.
63° F.	25·101	28·31	710·61	1·1278	1240°
,,	23·016	25·33	582·99	1·1005	1160
,,	18·578	18·327	340·48	·9864	960
,,	16·997	15·794	268·45	·9292	875
,,	15·098	13·410	202·47	·8882	775
,,	12·796	10·132	129·65	·7918	605
,,	11·06	7·599	84·044	·6870	440
,,	9·454	5·662	53·530	·5988	295
,,	7·518	4·097	30·780	·5452	180
,,	6·507	3·278	21·330	·5037	130
,,	5·04	2·384	12·016	·4730	—
,,	3·217	1·371	4·407	·4258	—
65°	26·856	31·29	840·33	1·1651	1290

The results given in the four tables are plotted out on the curve marked (A). The abscissæ give the rate at which the energy of the current is converted into heat, and the ordinates the corresponding resistance of the wire.

To determine the temperature of the wire corresponding to each resistance, another series of experiments was made, which are described hereafter. The values of α, β, and γ obtained were—

$$\left.\begin{array}{l}\alpha=0\cdot0119\\ \beta=0\cdot00112\\ \gamma=0\cdot512\end{array}\right\}$$

hence
$$\frac{r_t}{r_0}=\cdot0119\mathrm{T}^{\frac{1}{2}}+\cdot00112\mathrm{T}+\cdot512;$$

where r_0 is the resistance of the wire at the freezing point. By giving to T various values in this formula, a curve can be constructed showing the relation between the resistance and absolute temperature. Such a curve was drawn, and approximated for high temperatures to a straight line, as evidently must be the case from the form of the equation. By solving the equation for the maximum value of $\frac{r_t}{r_0}$ observed, it was found that the temperature of the wire when bright red hot was about 1100° C. It is known that platinum wire melts at approximately 1800° C.

The curve of relation between the temperature of the wire and the

electrical energy absorbed can now be constructed. Taking the abscissæ of the curve proportional to the watts absorbed, and the ordinates proportional to the temperatures in degrees Centigrade, the dotted curve marked B represents the relation between the power and the temperature for the results given in the tables.

I have sought to express this relation by an empirical formula in order to carry the curve to still higher temperatures. The equation—

$$\text{Temperature} = A\ (\log x)^2 + B\ (\log x) + C,$$

where x represents watts, agrees with the experimental results. The constants A, B, C have the values,

$$A = -63.$$
$$B = 1177.$$
$$C = -1603.$$

Mr. McFarlane, in a paper communicated to the Royal Society on January 11th, 1872, has arrived at the equation—

$$\text{Rate of energy} = a + bt + ct^2,$$

where a, b, c are empirical constants and t is the difference of temperature, from his experiments made through a very limited range of temperature, viz., about 60° C. ("Proc. Roy. Soc.," vol. 20, p. 90, 1872). Professor James Dewar, from experiments extending from a temperature of 80° to the boiling points of sulphur and mercury, also deduces a parabolic formula. ("Proceedings of the Royal Institution," vol. 9, p. 266.)

Making use of the equation I have given, the rate of energy absorbed for a temperature of 2780° C., is 155,000 watts, or sixty-seven times the rate of absorption at a temperature of 1670° C. Since 1670° C. is not much below the temperature of an incandescent filament (reverting to Sir William Thomson's calculation for the ratio of the radiant power per unit of surface of the sun to that of the incandescent filament), the temperature of the sun comes out to be about 2780°; which is in very close agreement with my former estimate based on other grounds. The effect of absorption between the sun and the earth would bring the two estimates into still closer agreement.

If we attempt to form a natural equation to the curve, it is apparent that it will consist of two terms—

(i.) The term due to radiation,

(ii.) The term depending on the convection and conduction of the air. The conduction of heat by the wire into the terminals may be neglected, as by taking a considerable length it becomes a small quantity of the second order. The first term I take to be proportional

to some power of the absolute temperature, the second may for the present be represented by $mF(t)$. Hence we have—

$$\text{Rate of conversion of energy} = AT^n + mF(t).$$

According to Prevost's theory of exchanges, the hot body is itself receiving radiant energy from the surrounding bodies; hence the radiant energy is more appropriately represented by $A(T^n - t^n)$, where t is the temperature of the surrounding bodies. Similarly it would appear probable that the conduction and convection will depend on the difference of temperature. Hence

$$\text{Rate of energy} = A(T^n - t^n) + mF(T - t).$$

The constants A and m will depend on the nature of the radiating body and on the surrounding medium.

Although for theoretical purposes it is important to eliminate the conduction and convection, yet in most cases a medium is present, and it has been shown by Mr. Crookes that, within limits, variations in pressure have only a very small effect on the amount of heat lost by conduction and convection.

I have not as yet been able to make any experiments on the determination of the term $mF(T-t)$, but it is my intention to make further investigations on this point. I am indebted to Professor Stokes for suggesting a method which appears to me likely to yield useful results. He proposes to construct a chimney of white paper, and to fix it over the wire through which the current is passing. The chimney will collect all the heated air ascending by convection, and by suitable means its temperature and the rate of flow can be measured, and hence the rate of loss of heat by convection estimated.

It might be supposed that conducting the experiment *in vacuo* would diminish the convection. According to the original researches of Dulong and Petit, the rate of cooling diminished in a geometrical progression, whose ratio was $\frac{1}{1 \cdot 366}$, as the pressure diminished in a second geometrical progression, of which the ratio was $\frac{1}{2}$. Mr. Crookes, in a paper communicated to the Royal Society ("Proc. Roy. Soc.," 1880, vol. 31, p. 239) described some experiments on this point, and showed that a diminution of pressure from 760 millims. to 120 millims. had a very slight effect on the convection. From 120 to 5 millims. the effect was somewhat more marked. A reduction of pressure from 5 millims. to 2 millims., however, produced twice as much fall in the rate of cooling as the whole exhaustion from 760 millims. to 1 millim. Hence to eliminate the effect of convection a very high exhaustion must be obtained.

It still remains to describe the experiments by which the constants

α, β, γ of the empirical formula connecting the resistance of the wire with its absolute temperature were determined. The wire was enclosed in a glass tube, stopped at either end with a plug, through which the wire passed centrally. The tube was fixed in a metallic trough, with an aperture in its cover sufficiently large to admit a mercurial thermometer placed in contact with the tube. In the first instance, the trough was filled with melting ice, and the resistance of the wire measured by a Wheatstone bridge. The ice was then removed, and two Bunsen burners were placed below the trough, and the temperature gradually raised by increasing the pressure of the gas in the burners.

In this way a series of simultaneous observations were made of the temperature of the wire and its corresponding resistance up to 100° C. The results are given in the subjoined table. Care was taken at each reading that the thermometer had become stationary, and really represented the temperature of the wire. A second series of observations were taken as the wire cooled from 100° to zero; and the results are likewise given in the table.

Temperature rising.			Temperature falling.		
Temperature.	Resistance ohms.	$\frac{r_t}{r_0}$	Temperature.	Resistance ohms.	$\frac{r_t}{r_0}$
0° C.	·5847	1·0000	100° C.	·6827	1·1680
0	·5837	—	97·7	·6815	1·1660
0	·5827	—	95·5	·6798	1·1631
0	·5827	—	90·0	·6741	1·1533
66·3	·6467	1·1064	78·5	·6619	1·1324
66·6	·6469	1·1068	76·6	·6601	1·1294
67·2	·6477	1·1081	62·5	·6463	1·1057
68·5	·6547	1·1201	48·3	·6308	1·0792
70·2	·6557	1·1218	46·6	·6299	1·0777
72·2	·6567	1·1235	32·2	·6147	1·0517
81·6	·6597	1·1286	31·6	·6140	1·0505
85·0	·6657	1·1389	21·6	·6052	1·0354
86·1	·6697	1·1458	0	·5857	1·0000
93·2	·6727	1·1509	0	·5857	—
95·0	·6747	1·1543			
98·8	·6777	1·1594			
99·5	·6817	1·1663			

For the reduction of the 26 equations obtained from these observations, the method of least squares was employed, giving

$$\alpha=0\cdot0119$$
$$\beta=0\cdot00112$$
$$\gamma=0\cdot512$$

The following are the results in substituting for the platinum a wire of platinum with 20 per cent. of iridium.

Diameter of wire ·73 to ·75 millim. Temperature of room 59° F.
Length of wire 100 centims. Current increasing.

Ampères.	Volts.	Watts.	Ohms.	Corresponding temperature of wire.	Condition.
2·169	1·638	3·553	·7552	—	Just warm.
4·652	3·045	14·165	·6546	—	Warm.
6·858	6·815	46·742	·9936	442°	Hot.
10·17	11·745	119·48	1·1545	725	Chars cotton.
11·477	14·21	163·09	1·2381	873	Dark red.
12·932	16·67	215·58	1·2891	965	Red.
15·198	22·04	334·97	1·4502	1252	Light red.
17·807	29·00	516·40	1·6286	1587	Yellow.
20·791	36·25	753·67	1·7436	1787	White.
Current decreasing.					
16·762	24·65	413·19	1·4706	1289	
14·210	19·865	282·28	1·3980	1160	
11·828	14·935	176·65	1·2627	918	
10·62	12·76	135·51	1·2015	806	
8·40	8·845	74·299	1·0530	545	
5·487	4·93	27·051	·8985	279	
4·338	3·625	15·725	·8364	—	

A second series were taken with the same piece of wire, and the current increased until the wire broke.

Ampères.	Volts.	Watts.	Ohms.	Corresponding temperature of wire.	Condition.
2·743	1·907	5·23	·6952	—	Just warm.
7·062	7·005	49·47	·9919	439°	Hot.
10·492	12·66	132·86	1·2066	816	Chars cotton.
15·634	23·69	370·38	1·5153	1372	Light red.
19·324	33·53	647·93	1·7351	1771	White.
21·044	37·99	799·47	1·8053	1899	
22·414	41·72	935·10	1·8613	2001	
23·913	45·19	1080·60	1·8898	2053	Incandescent.
25·475	49·91	1271·50	1·9592	2185	
26·33	53·70	1413·90	2·0395	2325	Broke into several pieces immediately after reading.

The relation between the resistance and temperature is given in the following table.

Temperature rising.			Temperature falling.		
Temperature.	Resistance ohms.	$\frac{r_t}{r_0}$.	Temperature.	Resistance ohms.	$\frac{r_t}{r_0}$.
	1·0072		Boiling water ...	1·0924	1·0852
Melting ice, 0° C.	1·0061	1·0000			
	1·0061		13·8° C........	1·0198	1·0131
12·1° C.........	1·0184	1·0117			
Boiling water....	1·0924	1·0852	Melting ice	1·0072	1·0000

The values for α, β, γ deduced by the method of least squares are—

$$\alpha = \cdot 005$$
$$\beta = \cdot 000694$$
$$\gamma = -\cdot 7285$$

In conclusion I have pleasure in acknowledging the assistance I have received in conducting the experiments, and in the preparation of this paper, from Messrs. E. Lauckert and Edward Hopkinson, D.Sc.

The Society adjourned over Ascension Day to Thursday, May 10th.

May 10, 1883.

Mr. JOHN BALL, M.A., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Dr. Dietrich Brandis (elected 1875) was admitted into the Society.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read from the Chair, as follows:—

Aitchison, James Edward T., Surgeon-Major, M.D.
Browne, James Crichton, M.D., LL.D.
Dobson, George Edward, Surgeon-Major, M.A., M.B.
Duncan, James Matthews, A.M., M.D.
Fitzgerald, Prof. George Francis, M.A.
Flight, Walter, D.Sc.
Frost, Rev. Percival, M.A.
Gill, David, LL.D.
Groves, Charles Edward, F.C.S.
Grubb, Howard, F.R.A.S.
Langley, John Newport, M.A.
Reinold, Arnold William, M.A.
Trimen, Roland, F.L.S., F.Z.S.
Venn, John, M.A.
Walker, John James, M.A.

The following Papers were read:—

I. "Theory of Magnetism based upon New Experimental Researches." By Professor D. E. HUGHES, F.R.S. Received April 5, 1883.

In a preliminary note* I communicated to the Royal Society the formulated results of a lengthened series of experiments with the induction balance, and I now present the experimental evidences which led me to these conclusions.

From numerous researches previously made by means of the induction balance, the results of which I have already published, I felt convinced that in researches upon the cause of magnetism, I should have in it the aid of the most powerful instrument of research ever brought to bear upon the molecular construction of iron, as indeed of all metals. It neglects all forces which do not produce a change in the molecular structure, and enables us to penetrate at once to the interior of a magnet or piece of iron, observing only its peculiar structure and the change which takes place during magnetisation or apparent neutrality.

* "Proc. Roy. Soc.," vol. 34.

The induction balance, while being one of the most simple instruments as regards its construction, and most powerful as to its powers of appreciating minute differences in the molecular construction of metals, requires a lengthened previous practical knowledge of its use and powers in order to obtain zero readings for each experiment. Thus with it we can very easily obtain an effect, such as a marked difference between two pieces of iron of similar size and chemical composition; but to reduce these differences, separate and measure them by the zero method, is of a peculiarly difficult nature. Not only does the electromotive force vary with each piece of iron, but also the time of its discharge, the time or duration of the effect being more variable and more indicative of the molecular structure than the electromotive force. Again, the form of the induction balance must vary according to the nature of the experiment. In some cases, where it is desirable not to pass an electric current through the metal, four coils should be used, as in the first instrument I presented to the Royal Society in 1879.* In others we should use three, two, or but one coil, as in the instrument which I shall describe in this paper. In order to avoid complication I will only mention results which can be easily obtained by this most simple form of apparatus, results which I believe can only be attributed to the molecular nature of magnetism.

This theory has long been foreseen, and predicted in almost complete perfection as regards the rotation of the molecules, by many authors, the earliest of whose notices, and the most clearly defined as being very near the results obtained by myself, will be found in the remarkable work by De La Rive,† 1853, who in chapter iii, page 317, under the title of "Influence of Molecular Actions upon Magnetism produced by Dynamic Electricity," says: "We have seen that heat, tension and mechanical actions generally facilitate magnetisation. M. Matteucci has found that torsion and percussion, and mechanical actions, not only facilitate the magnetisation produced upon soft iron by a helix that is traversed by a powerful current, but they also contribute, when the current has ceased to pass, to the destroying the magnetism in a very rapid manner; the same philosopher has likewise observed that torsion, when it does not pass beyond certain limits, augments the magnetisation produced upon steel needles by discharges of the Leyden jar.

"M. Marianini, who has made numerous and interesting researches upon magnetisation, arrived at curious results upon the aptitude that iron bars may acquire of becoming more easily magnetised in one direction than in another."

* "Proc. Roy. Soc.," vol. 29, p. 56.

† "A Treatise on Electricity, in Theory and Practice." By Aug. De La Rive. London, 1853.

M. De La Rive sums up a series of interesting experiments in these remarkable words: "The whole of the magneto-molecular phenomena that we have been studying lead us to believe that the magnetisation of a body is due to a particular arrangement of its molecules, originally endowed with magnetic virtue, but which in the natural state are so arranged that the magnetism of the body that they constitute is not apparent. Magnetism would therefore consist in disturbing this state of equilibrium, or in giving to the particles an arrangement that makes manifest the property with which they are endowed, and not in developing it in them. The coercitive force would be the resistance of the molecules to change their relative positions.

"There remains an important question to be resolved:

"Are mechanical or other actions, disturbers as they are of the electrical state, able of themselves to give rise to magnetism?"

Du Moncel, in his remarkable work on Magnetism, 1857, sustained and developed the views of De La Rive, and later, Wiedemann in "Poggendorff's Annalen," 1857—1859, as well as in his remarkable "Lehre von Galvanismus," 1861, sustained a similar theory. Wiedemann has lately given a *résumé* of his researches in the "Lumière Électrique," Paris, January 28, 1882, where he says—

"Nous admettons que les metaux magnétiques sont composés de molécules qui ont une polarité magnétique; nous ne voulons rien préciser quant à la cause même de cette polarité, qu'elle provienne de la séparation des fluids magnétiques, des vibrations d'un milieu entourant les molécules ou mieux encore de l'existance de courants élémentaires.

"Un corps ainsi constitué n'aura pas, en général, de magnétisme libre, parceque les axes magnétiques des molécules seront dirigés dans tous les sens et maintenus dans leurs positions respectives par les forces moléculaires. Mais une force magnétique extérieure, telle qu'une hélice où passe un courant, leur donnera une direction générale.

"En poursuivant cette hypothèse, M. Weber a réussi à expliquer théoriquement l'accroisement de la magnétisme d'une barre soumise à l'influence d'une hélice aimantée jusqu'à un maximum.

"Nous supposons, en outre, que les molécules dans leur mouvement éprouvent une certain résistance qui les empêche de suivre complètement l'influence des forces qui agissent sur elles."

In my preliminary note I gave the formulated results of a lengthened series of researches upon magnetism by the aid of the induction balance. As these agree in all important points with the theory of De la Rive, 1853, I do not wish it to be considered as a new theory or conception of molecular rotation, but as a theory based entirely upon researches into all the conditions of magnetism, and one which clearly defines the conditions of polarity and neutrality.

Gilbert, 1600, remarked the influence of torsion, stress, and vibra-

tions upon magnetism, since which time numerous researches have been made by means of torsion.

Matteucci* employed induction currents, by means of which he observed that mechanical strains increased or decreased the magnetism of a bar of iron.

Wertheim† published a long series of most remarkable experiments, in which he clearly proves the influence of torsion upon the increment or decrement of a magnetic wire.

Wiedemann‡ published his interesting experiments upon torsion-flexion in relation to magnetism, and in his remarkable work, "Galvanismus," 1861, relates his discovery of magnetism produced in an iron wire upon the passage or after of an electric current. He also gives a molecular theory of magnetism, similar to that of De La Rive, 1853, except that Wiedemann supposes that neutrality is the result of a heterogeneous arrangement, thus differing completely from the symmetrical neutrality that I have defined.

Villari§ showed increase or diminution of magnetism by longitudinal pull according as the magnetising force is less or greater than a certain value.

Gore,‖ in numerous interesting experiments, shows the influence of electric torsion and the identity of molecular sounds.

Sir W. Thomson,¶ in a remarkable paper, shows the critical value of the magnetisation of iron, nickel, and cobalt under varying stress, and also the effects of longitudinal as well as transversal strain upon its electric conductivity.

Tomlinson** has recently shown completely the influence of strain upon the conductivity of all metals, and that strain produces a molecular change in their structure.

These employed each a somewhat different method, either by primary or secondary currents acting upon a galvanometer or the action of magnetism upon a magnetic needle.

This field of research has been so thoroughly examined that I should have hesitated before trying to reproduce the results by ordinary or similar means. The induction balance, however, seemed to me peculiarly adapted, from its extreme sensitiveness to molecular changes of structure, to analyse such changes as are produced by magnetism. I have put its powers to use in the following researches, and, as I have necessarily studied all the phases of magnetism, I have been

* "Compt. Rend.," t. xxiv, p. 301, 1847.

† "Ann. de Chim. et de Phys.," (3), t. l, p. 385, 1857.

‡ "Archives," t. xxxv, p. 39, et tome ii (nouv. période), p. 300.

§ "Poggendorff's Annalen," 1868.

‖ "Phil. Trans.," 1874.

¶ "Phil. Trans.," p. 55, 1879.

** "Phil. Trans.," Part I, 1883.

obliged to use similar means, such as torsion, and also what would be a repetition of several experiments tried long since by others, if it was not for the fact that the induction balance enabled me to observe all the effects of stress on iron by means far more sensitive than any hitherto employed, and notice effects which, without its aid, would pass unperceived. Some idea of this extreme sensitiveness may be inferred from the fact that if we balance two pieces of iron to a complete zero, the addition or subtraction of the $\frac{1}{100000}$ part of iron, or the smallest filing to a large balanced mass of iron, will give out at once loud tones, which we can measure and appreciate.

In my previous experiments upon molecular magnetism I made use of an iron wire, but I have found that in all experiments upon magnetism it is far preferable to use thin flat strips, known as hoop-iron; as we can then have any desired amount of surface in the form of a thin flat bar.

Jamin had previously recognised the importance of using thin and wide steel pieces for his well-known magnets. We can easily magnetise them to saturation, and where torsion is applied the contour of the spiral becomes visible. Again, not only is the magnetism more equally distributed throughout their mass, but we are enabled to study the conditions of neutrality without the constant hindrance of superposed magnetism, as we constantly find in the case of a wire or bar of iron.

I cannot, within the limits of this paper, describe all the varied forms of coils and methods of obtaining perfectly defined zeros, which are necessary in any varied research with the induction balance; description of some of these will be found in my already published papers. I will now describe a simple form which will suffice for the experiments herein mentioned, and the following diagram shows its electrical communications.

A coil, having a large aperture, is fixed to a board; two small abutments or supports, A′A″, at a few inches distance on each side of the coil, allow us to suspend or fix an iron band or strip passing through the aperture, which then becomes the core of an electromagnet. This forms the essential portion of the apparatus. The iron or copper strip rests upon the two supports A′A″, which are 20 centims. apart; at one of these it is firmly clamped by two binding screws, while the opposite end at A″ can turn freely. The strip of iron J′J″, upon which the researches are made, is 22 centims. long, and of any desired width and thickness; it is fastened by means of a binding screw B″ to the projecting end of an axle, which has a key or arm G, serving as a pointer moving upon a circle, and giving the degree of torsion which the wire may receive; a binding screw allows us to fasten the wire, after turning the pointer to any degree of torsion, and thus preserves the required stress as long as is necessary.

The exterior diameter of the coil D is $5\frac{1}{2}$ centims., and that of the interior vacant aperture $3\frac{1}{2}$ centims., the width is 2 centims. Upon this coil is wound 200 metres of No. 28 silk-covered copper wire. A compensating coil E, whose wires are (when at zero) perpendicular to the coil D, can be rotated upon its axle F by means of its index lever arm G, moving over a graduated circle not shown in the diagram. The two coils D and E are affixed to the same piece of board, but independent of the board upon which rest the abutments A'A″, so

that they can, as desired, be moved to any portion of the strip of iron, in order that different portions of the same strip may be tested under a similar stress.

The coil D is joined to a telephone J or a sensitive galvanometer, whose terminals are reversed at each make and break of the current, and we may either pass the current in the manner described or may reverse all the communications, passing the current through the coil instead of the wire, and listening with the telephone to the induced surrent upon the iron wire alone.

The phenomenon of molecular movement upon the passage of an electric current through a wire or a coil surrounding a bar of iron was discovered by Page, 1837, and De La Rive published several memoirs on this subject in the "Comptes Rendus," 1846, wherein he not only clearly demonstrates the molecular cause of the sounds emitted at each change of the current, but also foreshadowed the theory which he published later in his "Treatise on Electricity," 1853. These movements have since been studied in a variety of ways, and by different methods, but they are all based upon the discovery of Page; as these sounds accompany all the rotations produced by torsion, in many cases too feeble to be heard, but becoming clearly audible by means of the microphone.

In my paper on "Molecular Magnetism," 1881, I proved by three different methods the identity of these sounds with all the phenomena of rotation. In the induction balance we observe only by the angular displacement of the molecules upon its wire or strip of iron, reacting both upon its own wire and the exterior coil; and the currents obtained from 1 centim. length of wire are sufficient to be clearly heard in the telephone held 10 centims. distant from the ear, and this with a feeble current of one Daniell element; under these conditions we hear no sounds in the wire itself, but they at once become audible by increasing the electric current or by the use of the microphone. We cannot, however, analyse sounds obtained in this way, nor can we perfectly analyse the induced currents which Matteucci was the first to obtain, unless we reduce them to a zero by an induction balance, a zero from which it is perfectly easy to perceive and measure the slightest change in the molecular structure.

Before relating a few of the representative experiments, and in order to avoid repetition, I will repeat the theory I gave in my preliminary note, based entirely upon researches on magnetism by the aid of the induction balance.

Theory of Magnetism.

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles

and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar magnet.

2. That each molecule can be rotated in either direction upon its axis by torsion, stress, or by physical forces, such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules and their polarities arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules and their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in this direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism, we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

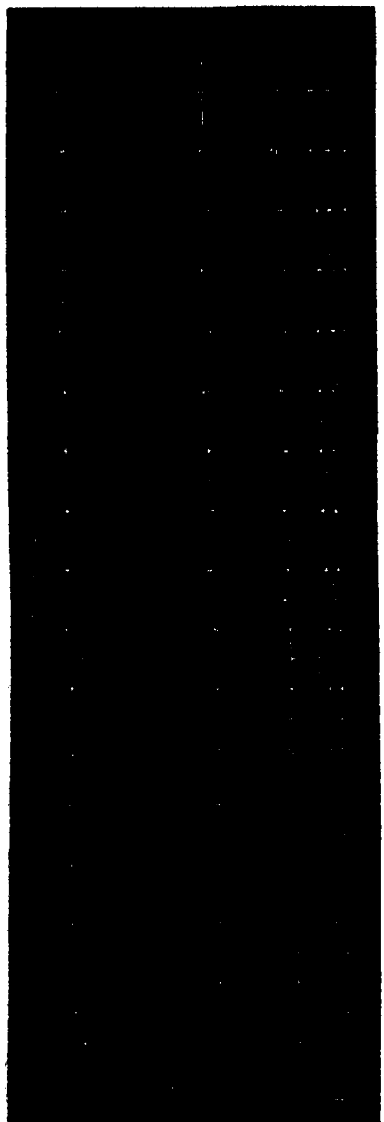
Experimental Evidences.

If in the induction balance already described we place an iron strip 1 or 2 centims. in width by $\frac{1}{2}$ millim. thickness, and pass rapid intermittent currents through it by means of the rheotome I, there is no induced current upon the coil D, as long as this strip, rod, or iron wire is entirely free from torsion, but the instant we apply the slightest torsion upon it by means of its key C, a very strong induced current in the coil gives loud tones in the telephone, which we can reduce to zero, and measure by the compensating coil E.

The phenomenon has this remarkable character: that a torsion of but $\frac{1}{360}$ part of a complete turn suffices to rotate the molecules to so great an extent as to be almost at the maximum of rotation that can be produced by torsion. I have demonstrated in previous papers* the phenomenon of molecular rotation, and that the induced currents so obtained are not direct from the central rod or wire to the coil, as the wires of the coil are perpendicular to the strip or rod of iron, and consequently at the zero of inductive effect; the induction from the central core upon the coil takes place through the medium of the rotated molecules whose angular displacement allows its reaction, both upon the coil and its central conducting strip, rod, or iron wire; a similar effect would take place if, instead of the rotated molecules, numerous oblong pieces of iron were rotated to a similar angle upon the conducting rod of iron. The induction or reaction of these could

* "Proc. Roy. Soc.," vol. 31, p. 525; vol. 32, pp. 25, 213, 1881.

take place at any degree which is not parallel or perpendicular to either the coil or its central conducting core, in direct proportion to its angular displacement.



The induced currents from molecular rotation are extremely powerful in proportion to the length of wire or strip of iron acting

upon the coil; and they have the distinctive feature that the maximum effect is obtained with an extremely feeble torsion, differing entirely from induction obtained through the medium of the spirality or increased parallelism of the conducting wire in relation to the secondary coil, as the effect here is a gradually increasing force, and exceedingly feeble at the degree of torsion sufficient to produce the maximum of molecular rotation. Thus we have a distinct phenomenon of molecular induction, its law of rapid maximum separating it completely from that obtained from non-magnetic metals, or from spirality of electric currents. If we compare the force obtained by molecular rotation with mere spirality of the electric current, we find that $\frac{1}{20}$ of a single turn gives the maximum for iron, whilst for a conducting copper wire it would require a spiral of similar diameter of fifty whole turns to equal or balance the power obtained from molecular induction, thus the effect is 900 times greater for the same degree of spirality; and if we neglect its distinctive feature of rapid maximum and compare it with ordinary electro-magnetic force, we find that it requires upon an iron core fifteen whole turns of similar conducting insulated copper wires to produce the same force; or for the same degree of spirality of conducting wires, the electro-magnetic induction thus formed is 270 times weaker than that of molecular induction. Thus we have not only its distinctive feature of rapid maximum, but an induced current which cannot be imitated nor accounted for except on the hypothesis of molecular rotation.

It will be seen from the following diagram that the rotation takes place very rapidly with the first degree of torsion, and after 20° or $\frac{1}{18}$ of a complete turn shows only the increased effect due to continued spirality.

It is evident from the above diagram that the molecules are rotated to their maximum during the first 20°, or in reality during the portion within the limits of elasticity; from this point the molecules become rigid by the strain of torsion, and they are then only rotated directly as the spirality of the wire or rod. The diagram only shows the effect of a right-handed torsion, the opposite torsion giving equal though opposite electric currents.

If the wire is free from strain, no increase of electric current changes its zero; nor does it while under torsion, though we may then produce a confused zero, when a powerful current is used, as the electromagnetic effects from its centre free from strain superpose themselves upon those due to rotation. For this reason we should not employ more than one small bichromate cell, as its most powerful effects are obtained with a comparatively weak primary current.

Knowing this, we observe that, after the passage of an extremely strong current (which may be in the same or contrary direction), and return to our previous feeble current, we have exactly the same

induced force as before, and with the same clear zero. Thus the molecules are not only rotated by torsion as already shown, but they have an inherent polarity which cannot be augmented nor destroyed.

We may, however, increase the molecular rotation and consequent force obtained by the application of heat, thus allowing greater molecular freedom. Annealing increases the effect in iron in a marked degree, and alloying has a marked influence in rendering the molecules of iron more rigid. The induction balance allows us to appreciate this differential freedom in different varieties of iron and steel, and in a future paper I shall show how this phenomenon of rotation can be applied to important practical results by investigations into the chemical nature of different varieties of iron and steel, and show distinctly the separating line of iron and steel.

Molecular Inertia.

A phenomenon of inertia is observed in these experiments, which I regard as not only being a proof of rotation, but of possession by the molecules of true inertia. For when a slight torsion of 20° has been applied to the strip or rod of iron, and we return it slowly to its zero of torsion, we have a remaining rotation of the character of residual magnetism; this is generally about a quarter of the maximum effects and of the same polarity, the rod then requiring a momentary mechanical vibration in order to allow the molecules freedom to return, which they at once do to an absolute zero; we have here a lagging behind which is characteristic of inertia. We have, however, more evident proofs, for if instead of freeing the rod gradually from torsion, we allow it to spring back suddenly, then the molecules continue their rotation by their acquired velocity far beyond zero, producing in most cases (where the rod of iron is very soft) a contrary polarity of fully one-half of its previous value, and although the rod is perfectly free from torsion, we must apply a slight torsion in the previous direction before obtaining a zero; we have here a zero under torsion evidently due to molecular inertia, for the instant we give the slightest mechanical vibration to the rod, the molecules return to their true zero, and now the slightest torsion produces its true polarity as before.

This inertia is far greater in soft iron than in hard iron or steel, being directly proportional to its softness, the consequence of this being that the time of rotation or discharge of soft iron is very slow compared with that of steel, and requires a compensation for time of discharge for each species of iron. I have already mentioned this as a difficulty in obtaining true zero observations, and I now make use of this very troublesome inertia to determine at once the degree of softness of any wire or rod.

We can understand this phenomenon when we know that while the

molecules in steel are excessively rigid, they have the quality of elastic rigidity, consequently the elastic pressure which prevented free rotation serves to restore them quickly to their previous zero, and also prevents any springing past or rotating from beyond their true zero.

Effects of Magnetism.

If the molecules of iron have inherent polarity and rotate freely upon their axes, we should expect that we could rotate them by the influence of an exterior permanent magnet alone, and this proves to be the case, for if (when the rod is free from torsion and the molecules are at zero) we approach a powerful compound permanent magnet perpendicular to the rod, the molecules rotate under its influence as freely as we have seen previously under torsion. Supposing the magnet to be at 20 centims. distance, and that we gradually lessen this distance, we find the maximum rotation whilst the magnet is at 5 centims., passing this point it gradually diminishes to a complete zero when distant 3 centims. the molecules now being parallel with the rod or the inducing coil according to the direction of rotation, and consequently (as before explained) no induced currents are possible. If we now continue to approach the magnet, the molecules are still further rotated, and we have now strong induced currents of the opposite polarity to the previous, notwithstanding that the rod is evidently magnetised continually in the same sense.

The rotation here, with its zeros and change of polarity, whilst the rod is gradually magnetised by increasing degrees of magnetic force, is due to the magnetic influence being perpendicular to the rod, allowing full rotation, from the circular neutrality which I shall explain later; but if the magnet is approached in the line of the rod, instead of perpendicular to it, we have continued increased rotation until it touches the rod; in this case we do not cross a zero, because the molecules can only turn in the direction of saturation.

Symmetrical Arrangement.

When we have a rod of soft iron, free from torsion, it is perfectly homogeneous in its structure, and we have a complete zero at all portions of the wire, rod, or strip of iron, at the extremities as well as at the centre. In order to observe this we should employ a very narrow coil, the one employed by myself being a single insulated wire, wound spirally upon itself upon a cardboard, being a marvel of workmanship, given me by Mr. A. Stroh; by means of this coil, whose thickness does not exceed $\frac{1}{10}$ millim., we can explore rods of any length, and if any portion is under strain we at once hear loud tones. Thus I find in all rods that have not been annealed, spots or places showing strains which have been caused by their mechanical treatment, such as hammering, rolling, or drawing into wire. We can by this

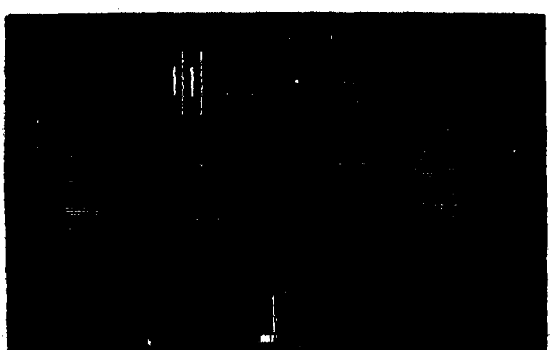
means not only perceive the strain but determine its direction by the polarity obtained. I have no doubt but that some day this will be practically applied to the appreciation of strains in iron shafts or cannons.

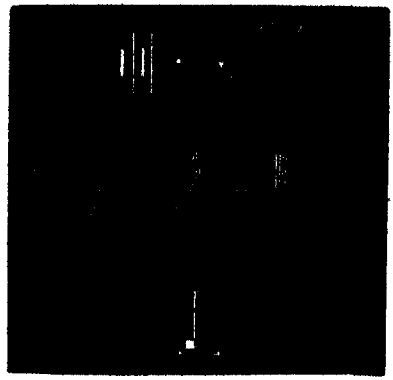
If we apply torsion to a soft iron rod, we find the degree of rotation equal at each section observed, quite independent of its length, consequently we find perfect symmetry of rotation throughout. If we magnetise the rod in such a manner as to leave its residual magnetism with several consequent poles, the portions into which the rod is thus divided are similar in their symmetry to several distinct magnets with their similar poles placed opposing each other, producing reversed induced currents at each consequent pole.

When we take observations by the induction balance, either upon electric conductivity or magnetism, we do not observe the effects as generally observed by other methods, viz., after the current has had time to reach its stable condition; we really observe the effects produced during the first instant of time, known as the variable period, observing from the first instant of electrical contact to a certain maximum, which can never be greater than the length of time of these contacts. These results are obtained by the rapid contacts of the rheotome, producing 500 vibrations per second, which the extremely rapid action of the telephone allows us to appreciate. Thus, the actual duration of the electrical contact is but $\frac{1}{1000}$ part of a second; and this proves the extremely rapid action of molecular rotation, as the effects which we have noticed are all produced in this fraction of a second.

Magnetic Conduction.

The following experiments were made by means of an induction balance of four and three coils, described fully in my paper, "Proc. Roy. Soc.," vol. 29, p. 56, 1879. The following diagram shows its electrical communications.





In fig. 3, two flat primary coils A′, A″ are near two similar secondary coils B′, B″, the distance between these coils being regulated by means of adjusting screws. The coils of B′, B″ are wound in contrary direction to each other, consequently the induced currents in each coil, if of equal strength, neutralise each other; as shown by the arrow, the induced currents act upon the telephone C. The primary coil A′ is joined through the battery D to the rheotome E and coil A″.

If we introduce a wire or bar of iron F′ in the coils A′, B′, the induced currents are increased by the magnetic conductivity from the upper to the lower coil, and as the coils A′, B′ and A″, B″ can be at any desired distance, or from 1 millimetre to 1 metre apart, we can test the conductivity of iron through any desired length. If we introduce into the second pair of coils A″, B″ a piece of iron of the same form, size, and molecular structure as that already in A′, B′, its effect on induced currents equals those of the first pair of coils and we have a perfect zero, but in practice we find that there is always some slight difference, even when the pieces are cut off the same bar; we have to compensate for this difference, and thus measure the differential structure.

In fig. 4 we have three coils, being the form adopted in my sonometer; the secondary coil B being equidistant from the primary coils A′, A″, acts upon the secondary coil in reversed direction, consequently a perfect zero of effect is found whenever the action of A′ equals that of A″, and this can be easily found by displacing either coil. If we introduce a bar of iron between the coils A′ and B, the balance no longer exists, owing to the greater electromagnetic conductivity on that side, but if the bar of iron F′ F″ is passed through the centre or axis of all the coils, as shown in the diagram, we have no effect except that due to a differential conducting power on one of its sides, the direction and force of which can be found by the amount of dis-

placement of the coil B necessary to find a zero. Thus, we can at once find the slightest strain or fissure, such as partial rupture in iron rods of any size or form. The coils may be close together or widely separated, and would find a practical application if applied to shafts undergoing constant strain, such as the screw shaft of steamboats.

The balance shown in fig. 3 is the one I generally employ, and is preferable for the following experiments.

If we take a flat disk of iron similar in form to our usual current coins, we find that if it is placed flat on or parallel with the coils, we have a great reduction of induced currents upon the pair of coils in which it is placed, due to the energy expended in creating the "Arago" circular currents, and its action then is precisely similar to copper or all non-magnetic metals, but if this disk is revolved 90°, or placed perpendicular, it then acts simply as a magnetic body and similar to the bar of iron F; the induced currents on its pair of coils are strengthened by the reaction of its electro-magnetic conductivity.

That conduction itself is due to molecular rotation is proved by the fact that soft iron shows a far higher conductivity than hard iron or steel; we observe here the same results of freedom and rigidity, and all the previous effects of rotation are again repeated as conduction. Soft Swedish charcoal iron shows such a marked superiority over all other irons and steel, as regards its power or the force obtained, that I feel convinced that the same superiority would be shown by its use for the cores of all electro-magnets, particularly those of telegraph instruments, where rapid action and the maximum of force obtainable from a feeble current are required.

Faraday showed that iron loses its magnetic force at red-yellow heat, and the induction balance is peculiarly adapted for investigating this phenomenon. If we place an iron wire or rod, as at F, we find on heating it that its conductivity gradually rises, being at black heat, just before the visible red, double of that noticed at the ordinary temperature, being a similar result to that previously noticed with the single coil balance. But if we increase the heat until the rod becomes red-yellow, all conductivity instantly vanishes and the iron apparently has lost all its powers, being then similar to a piece of copper or other non-magnetic metal; what takes place is, however, not destruction, for, at red heat, its inherent polarity reappears instantly with its full previous force. There seems no gradual diminution or reappearance of its polarity, it is sudden and apparently instantaneous, its time of action being less than the $\frac{1}{1000}$ part of a second. The induction balance will, no doubt, in the future, enable me to investigate this phenomenon.

I find that the conducting power of soft iron is greatly reduced by magnetising, generally one-fourth of its total conductivity, the residual magnetism being in reality a partial rotation, thus reducing

the available total amount of rotation. This is so evident from a series of experiments I have made on this subject, that I can safely predict that if an iron wire could be held charged to magnetic saturation, we should obtain no conduction whatever, and its action as regards the production of induced currents would be similar to copper; or, if the rotation were already complete and rigid, we should have no magnetic conductivity. We can observe this partial rotation in an iron rod, whose conductivity has been reduced by magnetising, as we have only to vibrate the wire, its molecules being rendered free instantly return to zero, and we have its previous full conducting powers.

The slightest torsion also reduces its conductivity, the greatest effect being on the first few degrees of torsion; thus torsion not only rotates the molecules during its elastic stage, but holds them imprisoned or fixed as rigidly as in hard iron or steel.

This form of induction balance allows us to demonstrate that polarity apart from the molecules does not exist. For if we balance one pole of a long magnetised iron or steel bar, we find the same conductivity for either pole, and the induced currents obtained are all in one direction, no matter if the coils act upon either pole or the apparent neutral centre; thus it is impossible with this form of balance to perceive any difference between north or south polarity. All that we observe is the degree of rotation of the inherent polarised molecules, and that this is symmetrical throughout is shown by the equal conducting power of all parts of a magnetised rod.

This form of induction balance also shows that the conducting powers of a bundle of fine iron wires and thin flat strips are equal, the thin band or strip of iron being superior to the fine wires if they are closely pressed together, as the maximum of induced rotative effect in all magnetic bodies is at or near the surface, consequently, a tube of iron will give greater power for a feeble force than a solid bar.

The time of discharge during which the molecules rotate or return to zero is comparatively short in bundles of loose iron wires, flat strips, and thin tubes, but exceedingly slow in solid bars. This confirms the results obtained by other methods, and which have already received practical application.

Visible Effects of Magnetism.

I have been able to repeat the greater portion of my researches with the induction balance, by simply observing the effects produced by magnetising iron and steel upon a magnetic direction needle according to the indications already given by the balance; by this means we are enabled to render visible most of the effects, but cannot so well determine the cause; in fact, without the aid of the induction balance all researches upon magnetism must remain incomplete. We

can, however, perceive the result of a supposed cause, and, as De la Rive has done, base a theory upon molecular rotation; but if we have previously analysed these movements by the aid of the induction balance, the following experiments will render visible effects due to molecular rotation.

The apparatus needed is simply a good compound horseshoe permanent magnet, 15 centims. long, having six or more plates, giving it a total thickness of at least 3 centims. We need a sufficiently powerful magnet, as I find that I obtain a more equal distribution of magnetism upon a rod or strip of iron by drawing it lengthwise over a single pole, in a direction from that pole as shown in fig. 5, as we can then obtain saturation by repeated drawings, keeping the same molecular symmetry in each experiment. A few well suspended magnetic needles of different powers are required, and as the needle itself induces magnetism, all observations should be made as far as possible by means of repulsion, repulsion being the only certain test (whenever the magnetism is feeble) that the iron or steel possesses independent polarity from that induced by the needle.

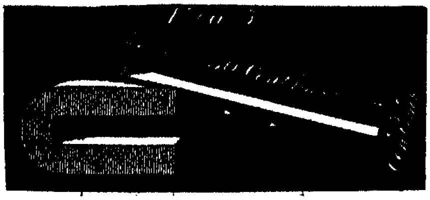
We should have also several flat strips of hoop-iron or thin band-iron, varying in width from 1 to 3 centims., and at least 30 centims. long, in order to observe one polarity free from the disturbance caused by the other. I find rods ½ millim. in thickness are the most easily magnetised to saturation, and this is the size of ordinary common hoop or band iron sufficing for the following experiments.

If we magnetise one of these rods by drawing it over one of the poles of the permanent magnet, we find that it is strongly magnetic; but if we apply a few slight elastic torsions the magnetism rapidly disappears. This effect has been observed and numerous researches have been made on this subject since Gilbert, 1600. Wertheim, 1857, in the "Annales de Chimie," has given a detailed account of his experiments upon the diminution of magnetism by torsion.

The diminution depends entirely upon the freedom of its molecules, for if we take a pure Swedish charcoal iron well annealed, a single slight torsion completely renders the rod free from magnetism, the molecules at once rotate to neutrality. I have in my possession, and will demonstrate with it at the reading of this paper, a solid bar of iron, 2 centims. diameter, 40 centims. in length, which, when strongly magnetised, and acting strongly upon the magnetic direction needle, loses instantly all its apparent residual magnetism by the simple torsion produced by the fingers upon its surface. Hard tempered steel loses but 2 per cent. by continuous and violent torsion, its molecular rigidity is so great that we cannot rotate the molecules by any mechanical strains. We may thus roughly estimate the degree of molecular freedom by the number of torsions required to render it

neutral; ordinary hoop iron requiring generally eight double right and left elastic torsions before it is nearly neutral; there will be, however, still a slight residual magnetism, which can be instantly rendered neutral by a slight torsion given to the rod when held vertically; or we may reverse the residual magnetism by the same directive influence of the earth's magnetism.

I have found it convenient to attach two brass clamp keys to the extremities of the rods, or simply turn the ends at right angles, as shown in the following diagram.



Wertheim, 1857, remarked that if we magnetise a wire under torsion, there will be a diminution upon freeing it from torsion, a still greater upon giving it a contrary torsion; and that its original force is in a measure restored by returning to the torsion under which it was magnetised, designating this phenomenon as "La rotation du maximum de magnétisme," and it is difficult to explain this phenomenon except upon the hypothesis of molecular rotation.

Wiedemann discovered that a wire becomes magnetic on or after the passage of a current, and in his "Galvanismus" he points out its molecular character. Sir W. Thomson* expresses his opinion that the effects are due to the outside twist of the wire forcing the current to pass round a fixed centre, and, consequently, that the effects can be explained as ordinary electromagnetism. This view evidently takes notice of the spiral action of the current alone. I have repeated this experiment, and find that the magnetism increases directly as the electromotive force, so it would be difficult to infer that the action of the current is due to molecular rotation, if it was not for the fact that the wire is magnetic after the passage of the current, as can be rendered visible by torsion, consequently I believe Wiedemann was fully justified in regarding this effect as one of molecular rotation.

Dr. Hooke, 1684, remarked that steel or iron was magnetised when heated to redness and placed in the magnetic meridian. I have slightly varied this experiment by heating to redness three similar steel bars, two of which had been previously magnetised to saturation and placed separately with contrary polarity as regards each other, the third being neutral; upon cooling, these three bars were found to have identical and similar polarity. Thus the molecules of this

* "Phil. Trans.," 1879, p. 55.

most rigid material—cast steel—had become free at red heat, and rotated under the earth's magnetic influence, giving exactly the same force on each, consequently the previous magnetisation of two of these bars had neither augmented nor weakened the inherent polarity of their molecules. Soft iron gave under these conditions by far the greatest force, its inherent polarity being greater than that of steel.

The gradual rotation of the molecules may be observed by strongly magnetising a soft iron wire. If we then pass a feeble electric current we find a slight diminution of magnetism after its passage, and as we increase the force of the electric current the molecules almost entirely rotate to zero; and if we heat the wire to redness, or simply put it in mechanical vibration, we have at last perfect neutrality, the current having rotated the molecules as it does an exterior needle perpendicular to itself, being simply Oersted's discovery of external rotation of the needle applied to the molecule itself. The wire when thus rendered neutral can, as Wiedemann remarked, again show evidence of magnetism. The neutrality here is that known as chain or circular magnetism, each molecule forming a link of a chain whose circle of attractions is completed around the axis of the wire through which the current has passed.

In order to perceive the change of polarity which torsion produces when an electric current is passing through an iron wire, the wire, which may be 2 millims. diameter, and 40 centims. in length, should be placed horizontally east and west, the centre of the wire being 3 or more centims. above the axis of the needle, thus differing completely from the position of the wire in Oersted's discovery, as in the latter case the maximum rotation is obtained when the wire is parallel with the needle; but in the following experiments the maximum effect is obtained when the needle is at right angles to the conducting wires, and none whatever if parallel, its action being perpendicular to the exterior rotations of Oersted's discovery; and a similar phenomenon will be observed in all the rods mentioned later in the case of superposed magnetism.

An iron wire, under the conditions above mentioned, shows strong north or south polarity by a left or right handed elastic torsion; and if we leave the wire with a slight remaining permanent torsion, producing movement of the needle to the left, it diminishes upon breaking the electric circuit, and increases to its previous value on re-establishing the current; this is due to a slight mechanical molecular twist. If we now magnetise this rod slightly in a contrary direction (the needle then having a movement to the right) we find that upon re-establishing the current it now increases the right deflection. Thus the slight magnetism has completely reversed the influence of the previous mechanical torsion.

The question here arises, have we rotated the molecules from their previous position, or is it simply the reaction of magnetism considered

apart from molecular rotation, which has changed the spiral direction of the electric current? The answer to this is most clear—that it is entirely due to molecular rotation. If the wire is perfectly neutral, we obtain by the current of one half-pint bichromate cell 45° deflection of the needle to the left for a left-handed torsion, and a similar degree to the right for a right-handed torsion, depending altogether upon the softness of the iron for the degree of force obtained, but in all cases perfectly equal on each side. If we magnetise the wire so that it produces about 45° to the right, then a torsion to the right produces no effect whatever; the molecules have already rotated to the degree which mechanical torsion could produce, and torsion aided by the electric current can produce no further rotation. The left-hand torsion, however, preserves its full maximum effect, and restores the molecules to their previous position. If again we magnetise the wire to saturation, so that the needle is violently moved to the right, having 80° or more deflection, then the right-hand torsion, instead of augmenting it, instantly reduces the deflection to 45°, being the maximum allowed by a right-handed torsion. We have here, in the case of an electric spiral, decreased effects from an increased mechanical torsion in the same direction, and the experiment clearly shows the rotation of the molecules independent of mechanical directive action.

Being desirous of noticing the effects of powerful electric currents, Dr. Warren De La Rue, F.R.S., kindly aided me by passing a current from his well-known chloride of silver battery through iron and steel wires. A condenser, 42·8 microfarad capacity, charged by 3,360 cells, was used. We first passed this enormous electric charge longitudinally through the permanent magnet I have described, fig. 5, completely destroying its evident polarity by rotating the molecules to neutrality.

This experiment was followed by discharging the condenser through magnetised and neutral steel knitting needles with a similar result. Many authors have stated that a steel wire or needle became magnetised by the charge from a Leyden jar, and as this is contrary to the neutrality I have spoken of, we continued the experiments with a view to determine the conditions of such magnetic effects. We have found that the magnetisation supposed to be the result of the electric spark is in reality due to the direction in which the needle lies at the time of the passage of the spark. Thus wires, when held perpendicular to the magnetic meridian, or east and west, became perfectly neutral, even when previously strongly magnetised; but if they were held in the magnetic meridian, a feeble magnetism was always produced, the direction of which was due entirely to the earth's polarity, and had no relation whatever with the direction of the current. If, however, the wires were twisted, as in a rope, then we had electromagnetic effects, due to the direction of the twist and current.

Similar but stronger effects were observed on soft iron wires. A neutral iron wire, 3 millims. diameter and 40 centims. in length, was held vertically, to represent a lightning-rod, the enormous spark or charge from the condenser, charged by the 3,360 cells, striking its upper portion. This rod became invariably magnetised by the earth's directive influence, its upper portion being in all cases south and its lower north. The magnetism, however, was comparatively feeble, and only equal to that obtained on the same wire by mechanical vibrations caused by repeated blows on the upper portion of the wire with a wooden mallet. From these experiments we draw the conclusion that every lightning-conductor must be magnetised, but that if it is without spirality the magnetism can never be greater than that due to the directive influence of terrestrial magnetism.

If we could destroy or change the inherent polarity of iron, we should expect to find traces of it in wires which had been submitted to such an enormous electric current, but the wires thus acted upon remain similar in every respect to ordinary iron wires, their polarity for a given force and their neutrality being similar in nature and degree; consequently the inherent magnetic polarity has not been changed or destroyed.

Neutrality.

The induction balance is affected by three distinct arrangements of molecular structure in iron and steel, by means of which we have apparent external neutrality.

In fig. 6, at A, we have neutrality by the mutual attraction of each pair of molecules, being the shortest path in which they could satisfy their mutual attractions. At B we have the case of superposed magnetism of equal external value; rendering the wire or rod apparently neutral, although a lower series of molecules are rotated in the opposite direction from the upper series, giving to the rod opposite and equal polarities. At C we have the molecules arranged in a circular chain around the axis of a wire or rod through which an electric current has passed. At D we have the evident polarity induced by the earth's directive influence when a soft iron rod is held in the

magnetic meridian. At E we have a longitudinal neutrality produced in the same rod when placed magnetic west, the polarity in the latter case being transversal.

In all these cases we have a perfect symmetrical arrangement, and I have not yet found a single case in well-annealed soft iron in which I could detect a heterogeneous arrangement, as Wiedemann supposes, in the case of neutrality.

We may observe this symmetry without the aid of the induction balance; for, if we magnetise a rod under torsion (which we have already shown produces an ultimate rigidity), and observe the effects upon a directive needle, we find that every portion is equally magnetised, and in the same symmetrical arrangement of its molecules. We can also observe the probable existence of the double molecule, A, fig. 6, by magnetising a rod with feeble magnetic power. We then find a certain degree of magnetism, which is of exactly the same force as that produced when magnetised under a right-hand torsion, and if now we again magnetise it under a left-hand torsion, we have still the same force. This, without the existence of a double molecule, would have required a far greater inducing magnetic power to have rotated the molecules to the same degree under a contrary torsion. The limits of this paper do not allow me to bring further proofs of the existence of the double molecule, which the induction balance plainly indicates, as it requires a separate paper on this subject alone. If we consider the fire magnets of Dr. Hooke, we know that we cannot produce neutrality by heat, that we cannot melt or manufacture iron, except under the influence of the earth's directive influence, and that we can only produce apparent neutrality by vibrations or torsions, allowing, by the molecular freedom thus given, a complete closed circle of attractions, as at A or C, and that a similar apparent neutrality arises from the superposed magnetism of B, or the tranversal F, both of which in small wires or rods are apparently completely neutral.

We can produce a perfectly symmetrical closed circle of attractions of the nature of the neutrality of C, fig. 6, by forming a steel wire into a closed circle, 10 centims. diameter, if this wire is well joined at its extremities by twisting and soldering. We can then magnetise this ring by slowly revolving it at the extremity of one pole of a strong permanent magnet, and to avoid consequent poles at the part last touching the magnet, we should have a graduating wedge of wood so that whilst revolving it may be gradually removed to greater distance. This wire will then contain no consequent points or external magnetism, it will be found perfectly neutral in all parts of its closed circle; its neutrality is similar to C, fig. 6, for if we cut this wire at any point we find extremely strong magnetic polarity, the wire being magnetised by this method to saturation and having retained (which it will indefinitely) its circle of attractions complete.

My researches upon the molecular structure of neutrality required various kinds of iron and steel; my friend, Mr. W. H. Preece, F.R.S., kindly used his influence with the wire manufacturers supplying the Government telegraphs in procuring for me sixty different samples of iron and steel of a similar gauge, but of different qualities, as the induction balance has since proved, and the results obtained will be given in a future paper upon the "Molecular Structure of Iron and Steel."

Superposed Magnetism.

Knowing that we can rotate or diminish magnetism by torsion, I was anxious to obtain a complete rotation from north polarity to neutrality, and from neutrality to south polarity, or to completely reverse magnetic polarity by a slight right or left torsion.

I have succeeded in doing this and obtaining strong reversal of polarities, by superposing one polarity given whilst the rod is under a right torsion with another of the opposite polarity given under a left torsion, the neutral point then being when the rod is free from torsion. The rod should be very strongly magnetised under its first or right-hand torsion, so that its interior molecules are rotated, or, in other words, magnetised to saturation; the second magnetisation in the contrary sense and torsion should be feebler, so as only to magnetise the surface, or not more than one-half its depth; these can be easily adjusted to each other so as to form a complete polar balance of force, producing when the rod is free from torsion the neutrality as shown at B, fig. 6.

If we now hold one end of this rod at a few centimetres distance from a magnetic directive needle we find it perfectly neutral when free of torsion, but the slightest torsion right or left at once produces violent repulsion or attraction, according to the direction of the torsion given to the rod, the iron rod or strips of hoop-iron which I use for this experiment being able, when at the distance of 5 centims. from the needle, to turn it instantly 90° on either side of its zero.

The external neutrality that we can now produce at will is absolute, as it crosses the line of two contrary polarities, being similar to the zero of my electric sonometer, whose zero is obtained by the crossing of two opposing electric forces.

This rod of iron retains its peculiar powers of reversal in a remarkable degree, which distinguishes it completely from ordinary magnetisation, for the same rod when magnetised to saturation under a single ordinary magnetism loses its evident magnetism by a few elastic torsions, as I have already shown, but when it is magnetised under the double torsion with its superposed magnetism, it is but slightly reduced by vibrations or numerous torsions; and I have found it impossible to render this rod again free from its double polar

effects, except by strongly remagnetising it to saturation with a single polarity, the superposed magnetism then becomes a single directive force, and we can then by a few vibrations or torsions reduce the rod to complete neutrality.

The effects of superposed magnetism and its double polarity I have produced in a variety of ways, such as by the electromagnetic influence of coils, or in very soft iron simply by the directive influence of earth's magnetism, reversing the rod and torsions when held in the magnetic meridian, these rods when placed magnetic west showing distinctly the double polar effects.

It is remarkable also that we are enabled to superpose and obtain the maximum effects on thin strips of iron from $\frac{1}{4}$ to $\frac{1}{2}$ millim. in thickness, whilst in thicker rods the effect is far less, as it is masked by the comparatively neutral state of the interior, the exterior molecules then reacting upon those of the interior, allowing them to complete in the interior their circle of attractions.

I have already mentioned several different forms of induction balance which are applicable to—1. Testing the softness of iron and steel; 2. Researches upon the cause of tempering in steel; 3. Finding the dividing line of iron and steel; 4. Finding mechanical strains in shafts, cannons, or steamboat screw shafts; 5. Showing unmistakably the best quality of iron for electro-magnets; 6. Researches upon all the phenomena of magnetism, and upon all questions relative to the molecular structure of metals.

I was anxious to show upon the reading of this paper some mechanical movement produced by molecular rotation, consequently I have arranged two bells that are struck alternately by a polarised armature put in motion by the double polarised rod I have already described, but whose position at 3 centims. distant from the axis of the armature remains invariably the same. The magnetic armature consists of a horizontal light steel bar suspended by its central axle, carrying at its extremities two glass beads 5 millims. diameter serving as hammers; the bells are thin wine glasses, giving a clear musical tone loud enough by the force with which they are struck to be clearly heard at some distance. The armature does not strike these alternately by a pendulous movement, as we may easily strike only one continuously, the friction and inertia of the armature causing its movements to be perfectly dead beat when not driven by some external force.

The mechanical power obtained is extremely evident, and is sufficient to put the sluggish armature in rapid motion, striking the bells six times per second, and with a power sufficient to produce tones loud enough to be clearly heard in all parts of the hall of the Society. As this is the first direct transformation of molecular motion into mechanical movement I am happy to show it on this occasion.

On the reading of my preliminary note I demonstrated by visible experiments many of the points of the theory I have advocated, and which I believe explains all conditions of magnetism, and I propose on the reading of this paper to demonstrate experimentally the remaining evidences.

II. "Remarks on the Soundings and Temperatures obtained in the Faeroe Channel during the Summer of 1882." By Staff Commander T. H. TIZARD, R.N., H.M.S. "Triton." Communicated by SIR FREDERICK EVANS, K.C.B., F.R.S. Received April 16, 1883.

[PLATES 4-8.]

Introduction.—The exploration of the Faeroe Channel commenced by H.M.S. "Lightning," in 1868, under the direction of Dr. Carpenter, F.R.S., the late Sir Wyville Thomson, F.R.S., and Mr. Gwyn Jeffreys, F.R.S., at the instance of the Royal Society,* revealed a remarkable peculiarity, namely, the fact that over one portion of that channel the temperature of the water at the bottom differed 12° to 14° F. from that obtained at similar depths in the other portion, and further investigation by H.M.S. "Porcupine" in 1869 confirmed the observations previously obtained on board the "Lightning."

The cause of this phenomenon appears to have been unsuspected at the time, but during the voyage of H.M.S. "Challenger" several such peculiarities were observed, though not to such a marked extent, and a theory was formed that where differences of bottom temperature existed at equal depths in adjoining areas those areas would probably be found separated by submarine ridges.

Viewing the question on board the "Challenger" from our own observations, combined with those previously obtained in the "Lightning," "Porcupine," and "Shearwater," and with the advantage of Dr. Carpenter's conclusions on oceanic circulation published in the "Proceedings of the Royal Society" for 1869, it seemed to us reasonable to suppose that in those areas where the minimum temperature was found constant from a given depth to the bottom over an area contiguous to another where the temperature decreased as the depth increased, those areas must be separated by a submarine ridge, as then the phenomena might be readily explained. For instance, the condition might arise (*a*) if the minimum temperature was the mean winter temperature of the coldest portion of the separated area, in which case the water at the surface would be flowing in, whilst below it would be flowing out over the submarine ridge, as seems to be the

* See "Proc. Roy. Soc." for 1868.

case with the Mediterranean and Red Seas; or (*b*) the minimum temperature might be that which exists outside the separated area at the lowest part of the submarine ridge, in which case the water would be flowing in at the bottom over the ridge, and out at the surface, as seems to be the case in the Sulu, Celebes, and Banda Seas.

As the voyage of the "Challenger" was devoted to general oceanic research, it was found impracticable to spend much time over particular localities without lengthening the voyage considerably, and consequently there was no opportunity of testing by actual soundings the correctness or otherwise of this theory. This seemed to be practically of very little consequence, as in the Faeroe Channel, close to our own shores, the same phenomenon existed, and a short time devoted to its further exploration would decide whether a submarine ridge there separated the two areas of different bottom temperatures, as was predicted would be the case in No. 7 of the "Challenger" reports published by the Admiralty; for, applying our views to the results obtained in the Faeroe Channel in 1868–69, we concluded that, as in both areas in that channel the temperatures agreed fairly well to a depth of 200 fathoms, whilst at greater depths a marked difference existed, we should find a submarine ridge across the channel with from 200 to 250 fathoms over it, and that as in the cold as well as the warm area the temperature at 200 fathoms exceeded the mean annual temperature of the 60th parallel of latitude, the whole body of the water was moving steadily to the north-eastward over the ridge.

The late Sir Wyville Thomson considered the Faeroe Channel as a test question, and consequently represented to the Hydrographer of the Admiralty (Sir F. J. Evans, R.N., K.C.B., F.R.S.), in 1880, the desirability of despatching a small vessel to obtain some soundings and other observations in this locality. The Hydrographer having recommended this project to the favourable consideration of the Lords Commissioners of the Admiralty, their Lordships sanctioned the small hired surveying vessel "Knight Errant" (employed on the west coasts of the United Kingdom) being sent to the Faeroe Channel, and during the month of August, 1880, a sufficient number of soundings and temperature observations were obtained to show that a submarine ridge existed, though the actual extent of the ridge was not determined. A full account of the results obtained in the "Knight Errant" was published in the "Proceedings of the Royal Society of Edinburgh," session 1881–82.

The existence of a submarine ridge having been ascertained, Sir Wyville Thomson represented to the Royal Society the advisability of more thoroughly investigating it by a series of cross-sections to determine the slopes on each side, and to ascertain with greater exactness the limit of the cold area and the nature of the bottom on this ridge. The Royal Society recommended Sir Wyville's views to the

favourable consideration of the Lords Commissioners of the Admiralty, but their Lordships, whilst agreeing that the exploration of the Faeroe Channel was very important, were unable to spare a vessel for the purpose during the summer of 1881, and, unfortunately, before the end of that year Sir Wyville, whose health had been undermined by exposure to the vicissitudes of climate during the voyage of the "Challenger," succumbed to a severe illness without being able to complete either the report of the voyage of the "Challenger," or the many investigations he had undertaken as bearing more or less on that voyage.

Shortly after the death of Sir Wyville, Mr. John Murray, one of the naturalists of the "Challenger" expedition, was selected to succeed him as the editor of the "Challenger" Reports, and, as he had accompanied the "Knight Errant" in her cruise to the Faeroe Channel in 1880, and was also of opinion that the exploration of that channel bore directly on the results of the voyage of the "Challenger," he again brought before the Royal Society the desirability for further investigating this submarine ridge, and at their instance the Hydrographer, with the sanction of the Lords Commissioners of the Admiralty, directed H.M.S. "Triton" to carry out this work, and Mr. Murray embarked in that vessel to assist in making the necessary observations.

Equipment.—The "Triton" being the surveying vessel newly fitted to take the place of the "Porcupine" on the south and east coasts of the United Kingdom, had every appliance on board necessary for the work, with the exception of dredges, trawls, and dredging line. Some dredges remaining from the stock returned by the "Challenger" were found available, and the Royal Society provided the trawls and necessary rope. All the instruments were of the pattern used in the "Challenger" expedition excepting one deep-sea thermometer which was an improvement on the ordinary type in use by Mr. Buchanan.

Narrative.—The "Triton" arrived at Stornoway on the 25th July, and between that date and the 4th September, made three trips to the Faeroe Channel, each trip being about ten days' duration. Notwithstanding the generally unfavourable condition of the weather experienced, five sectional lines of soundings were obtained across the ridge (which has been named after the late Sir Wyville Thomson), and numerous other soundings between these sectional lines, making a total of 135 soundings, 14 serial temperature soundings, and 17 hauls of the dredge or trawl.* The work of sounding and obtaining temperatures was proceeded with steadily on every occasion when the weather was sufficiently clear to admit of the position of the soundings being ascertained by astronomical observation; during misty or foggy

* See table, plan, and diagrams attached.

weather either the dredge or trawl were usually put out, or the tow nets lowered to such depths as required.

After completing the work in the Faeroe Channel, the vessel left Stornoway for Oban, and from thence proceeded into the Atlantic about 100 miles north-west of Ireland, to test some pressure gauges in connexion with the observations of Professor Tait on the thermometers of the "Challenger," for which purpose Professor Chrystal, of the University of Edinburgh, accompanied the ship on this section of the voyage. The "Triton" finally returned to Glasgow on the 17th September, and then resumed her ordinary surveying work.

The Wyville Thomson ridge.—The soundings obtained in the "Triton," combined with those formerly taken in the "Knight Errant," prove conclusively the existence of a submarine ridge in the Faeroe Channel, extending from the edge of the bank north of Rona Island to the fishing bank to the south-west of the Faeroe Islands. To the north-east of this ridge, the temperature of the water at depths exceeding 350 fathoms is under 32° F., whilst to the south-west of it the temperature at similar depths is above 42° F., excepting in one part, where, for a short distance south-west of the deepest part of the ridge, a drain of the Arctic water is carried across, and is sufficient to cool the bottom water below 40° for a distance of 8 miles from the axis of the ridge.

The general depths over the Wyville Thomson ridge, which is 100 miles in length, by 10 in width, are from 250 to 280 fathoms, with here and there shoaler heads. In one part, however, there is a saddle or gap 7 miles wide, where the depths are from 300 to 330 fathoms. On each side of the ridge the depths increase to 600 fathoms or upwards.

The indications given by the lead as well as the dredge and trawl, show that the Wyville Thomson ridge consists of stones and gravel, whilst to the north-east of the ridge, in the cold area, the bottom is of a hard blue mud, and to the south-west a softer gray mud.

The ridge seems to be a portion of a chain of hills, mostly submerged, which stretch irregularly from the bank off the north-west coast of Scotland to the Faeroe Islands, Iceland, and Greenland, for we know that depths of about 200 fathoms exists between the Faeroe Islands and Iceland, as well as between Iceland and Greenland. As oceanic soundings become more numerous, doubtless many more such chains of submarine elevations will be discovered, for there is reason to believe that the floor of the ocean is not so level as is generally supposed. The absence of mud on the top of the Wyville Thomson ridge may be accounted for by the water flowing over it, washing away all the small particles.

Plans and Diagrams.—To show the position and form of the ridge, a series of diagrams and a plan have been constructed. The plans shows

all the soundings obtained, as well as the position of the five sectional lines across the ridge, and the line of demarcation between the cold and warm areas, for which purpose the isotherm of 40° has been selected as the best distinctive mark. The diagrams show the temperature curves, and a profile of each section exhibiting the form of the ridge, and the distribution of temperature from the surface to the bottom.

The diagrams all appear to point to the same conclusion, thus agreeing with theory, namely, that the water is flowing steadily to the north-east over the ridge. For instance, in Plate 7, Section A, it will be seen that the curves of temperature begin to diverge rapidly below the depth of 170 fathoms, and by referring to Plate 5, Section A, it will be seen that the least depth over the ridge on this section is 120 fathoms. In Plate 7, Section B, it will be seen that the curves taken in the warm area and on the ridge, agree very closely, whilst that taken in the cold area, 10 miles north-east of the shortest cast obtained on this section, 260 fathoms, begins to diverge rapidly at 200 fathoms from the other two curves. In Plate 7, Section C, curves taken in the warm and cold areas are sensibly the same to the depth of 300 fathoms, and a reference to Plate 5, Section C, will show that on this section the least depth found on the ridge was 305 fathoms. In sections B, D, and E, where the least water on the ridge is much the same, the isotherm of 40° on each section at a distance of 10 miles from the axis of the ridge, is found at almost precisely the same depth, viz., 280 fathoms, or the precise depth of the ridge, whereas in Section C, where the depth of the axis of the ridge is 305 fathoms, the isotherm of 40° is found at a depth of 300 fathoms in the cold area, and in Section A, where the depth on the axis of the ridge is 120 fathoms, the isotherm of 40° is at a depth of 250 fathoms in the cold area. The depth then at which the isotherm of 40° is found in the cold area depends on the depth over the ridge. As before mentioned, in the warm area, all the temperatures exceed 40°.

The question then arises, if the water is flowing steadily over the Wyville Thomson ridge to the north-east, how is it the water at the bottom in the cold area retains its low temperature? This has hitherto been very difficult of explanation, as there was apparently no outlet for it over the ridge, and consequently we might expect that its temperature would be influenced by the mass of heated water above; for the excess of inflow in the Faeroe Channel might be altogether absorbed by the outflow, which we know is constantly in progress between Iceland and Greenland. The soundings and temperatures taken this year, however, led to the discovery of a slight outflow of the cold Arctic water over the deepest part of the Wyville Thomson ridge, in the 7-mile gap, which breaks the continuity of the 300 fathom contour-line of soundings. Here the cold water was

traced flowing across the ridge, and gradually increasing in temperature as it moved to the southward, until at a distance of 15 miles from the axis of the ridge, it was of the usual normal temperature of the warm area in that locality.

This outflow of cold water seems to affect all the bottom temperatures to the westward of Section C; for, whilst to the eastward of that section they are from 45° to 46° at depths of 500 fathoms, to the westward they are from 42° to 43°, that is 3° lower.

There is then apparently a regular interchange of the waters across the Wyville Thomson ridge, the Atlantic water flowing north-east into the Arctic basin on the surface, and as far down as the ridge permits, over the greatest portion, whilst over the deepest part of the ridge there is a small outflow of Arctic water into the Atlantic, which although of infinitely less volume than the water moving to the north-east, yet appears to be sufficient to enable the bottom water of the Arctic basin, immediately adjacent to the ridge, to retain its low temperature. Were there no other outlet to the Arctic basin, it is probable the outflow over the ridge at the bottom would equal the inflow at the surface, but, as before remarked, we know the surface water on the western side of the Arctic basin has a steady flow to the southwards along the coast of Greenland.

The existence of the Wyville Thomson ridge in the locality predicted, tends to prove the general correctness of the theory formed in the "Challenger," but farther observations in other localities where the same phenomenon exists, are requisite to determine its absolute correctness, more especially when we remember that in nearly every instance where the bottom temperatures differ materially in adjoining areas, the minimum temperature in one of those areas, the warm, is found at a considerable height from the bottom; whereas in the other area, the cold, the temperature decreases with the depth, the minimum being at the bottom. In the Faeroe Channel, however, the temperature in the warm area decreases as the depth increases, whilst in the cold area it remains almost constant at $30\frac{1}{2}$° F. at depths exceeding 350 fathoms, thus reversing the rule which obtains elsewhere. For instance, in the Mediterranean the temperature of the sea is constant at 55° F. at depths exceeding 100 fathoms, whereas in the Atlantic, the only sea in communication with the Mediterranean, the temperature outside the Straits of Gibraltar decreases as the depth increases. In the Red Sea the temperature is constant at 70° F., at depths exceeding 100 fathoms, whereas in the Indian Ocean it decreases with the depth. In the Sulu Sea the temperature is constant at 50°·5 F. at depths exceeding 400 fathoms, whereas in the adjacent seas the temperature decreases to 39°, and there are also considerable areas in the Atlantic, as well as the Pacific, where a minimum temperature is reached at a certain depth, whilst in adjoining areas the temperature

either decreases to the bottom, or a lower temperature is found at a similar depth. These differences, though slight, give reason for believing that the flow of water from the Antarctic is impeded by submarine ridges. The Arctic water is apparently quite cut off from the general oceanic circulation, excepting at the surface, and to a depth of 200 fathoms.

Tides.—When the weather was favourable, and the dredge or trawl was down, we noticed, more especially in the western part of the Faeroe Channel, a regular tidal set, the greatest strength recorded being three-quarters of a mile per hour. The direction of the tidal stream appeared to vary considerably, and unfortunately our opportunities for observations were few, for, as a rule, the long swell usually experienced entirely masked the tide, the "Triton" being so light, that on almost all occasions when the engines were stopped, even with the trawls down, the normal position was broadside to the swell. The height of the waves usually experienced was from 9 to 12 feet, but waves of 17 feet from trough to summit were not uncommon, and early in September, during a gale, they were recorded as 25 feet from trough to summit.

The highest wave recorded during the voyage of the "Challenger" was 23 feet from trough to summit.

At all times we noticed that the sea was shorter and heavier on the Wyville Thomson ridge than on either side, and sometimes when crossing it we observed peculiar "smooths," as if oil was floating on the surface, or a spring welling up from the bottom. In these smooths the temperature of the water remained unaltered.

Dredgings and Trawlings.—The result of the dredgings and trawlings, as well as of the surface dredgings, by the tow net, will be reported on by Mr. John Murray, who accompanied the "Triton" throughout her exploration of the Faeroe Channel.

Table I.—Soundings obtained in Faeroe Channel by H.M.S. "Triton," August, 1882.

No. of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
			° ′ ″	° ′ ″				°	
1	Aug. 4th	11.0 A.M.	59 34 30	6 36 30	200	Sand	B 0·5	49·3 49·3	Serial temperatures. See Table II No. 13
2	"	Noon	59 36 30	6 32 30	157	Sand	B 0·5	49·6 50·0	
3	"	0.50 P.M.	59 38 41	6 28 0	143	Sand	B 0·5	49·8 50·0	
4	"	1.40 "	59 40 56	6 23 15	129	Sand	B 0·5	50·0 50·0	
5	"	2.25 "	59 43 11	6 18 30	116	Sand	B 0·5	50·0 50·0	
6	"	3.7 "	59 45 22	6 14 3	145	Sand	B 0·5	50·0 50·5	
7	"	3.47 "	59 47 30	6 9 30	173	Sand	B 0·5	49·9 49·8	
8	"	5.0 "	59 48 30	6 21 0	190	Sand	B 0·5	49·5 49·5	
9	"	5.45 "	59 51 30	6 21 0	240	Sand and gravel	B 0·5	47·6 47·5	Dredging station.
10	Aug. 5th	6.0 A.M.	59 43 0	6 40 0	300	Sand	B 0·5	48·7 48·0	
11	"	10.0 "	59 37 30	6 49 0	530	Mud	B 0·5	46·2 46·2	Trawling station.
12	Aug. 7th	4.30 "	60 23 15	8 58 0	230	Stones	B 0·5	45·4 42·2	

No. of sounding.	Date.	Hour.	Position.		Depth in fathoms.	Nature of bottom.	Bottom temperature.		Remarks.
			Lat. N.	Lat. W.			No. of therm.	Result.	
			° ′ ″	° ′ ″					
13	Aug. 7th	5.20 A.M.	60 25 30	9 1 0	199	Sand and stones	B 0·5	47°·1 46·7	
14	"	6.10 "	60 28 0	9 4 0	165	Sand and stones	B 0·5	47·2 47·5	
15	"	7.0 "	60 31 0	9 7 0	158	Sand and stones	B 0·5	47·5 47·5	
16	"	7.45 "	60 33 15	9 10 0	147	Sand	B 0·5	48·5 46·0	
17	"	8.30 "	60 35 30	9 13 0	116	Sand and stones	B 0·5	48·2 48·0	
18	"	9.5 "	60 37 50	9 16 0	117	Fine sand	B 0·5	49·0 48·8	
19	"	10.0 "	60 40 0	9 8 0	91	Fine sand	B 0·5	50·1 49·8	
20	"	11.0 "	60 40 40	8 56 15	81	Fine sand	B 0·5	49·8 49·5	
21	"	Noon	60 41 14	8 45 0	77	Stones and shells	B 0·5	50·0 49·5	
22	"	0.40 P.M.	60 41 30	8 37 30	98	Stones and shells	B 0·5	49·5 48·5	
23	"	1.16 "	60 38 40	8 34 0	130	Hard ground	B 0·5	48·5 47·7	
24	"	1.57 "	60 36 10	8 31 0	205	Hard ground	B 0·5	46·8 46·4	
25	"	2.40 "	60 33 40	8 27 30	290	Gravel	B 0·5	39·8 38·8	

No. of sounding.	Date.	Hour.	Position.		Depth in fathoms.	Nature of bottom.	Bottom temperature.		Remarks.
			Lat. N.	Long. W.			No. of therm.	Result.	
			° ′ ″	° ′ ″				°	
26	Aug. 7th	3.32 P.M.	60 31 30	8 29 30	268	Gravel	B 0·5	45·4 44·8	
27	,,	4.20 ,,	60 29 14	8 30 0	320	Mud	B 0·5	36·4 36·4	
28	,,	5.10 ,,	60 29 14	8 23 30	365	Mud	B 0·5	31·8 31·8	
29	,,	6.40 ,,	60 26 0	8 33 0	261	Stones and shells	B 0·5	43·0 42·5	
30	,,	7.30 ,,	60 24 30	8 37 0	215	Stones and shells	B 0·5	47·3 46·5	
31	,,	8.15 ,,	60 23 0	8 41 0	205	Fine sand	B 0·5	47·5 47·0	
32	Aug. 8th	4.35 A.M.	60 39 30	9 6 0	87	Sand and shells	B 0·5	49·5 49·0	Trawling station.
33	,,	8.45 ,,	60 39 30	8 55 45	80	Stones and shells	B 0·5	47·8 48·0	Serial temperatures. See Table II, No. 14.
34	,,	9.40 ,,	60 37 0	8 53 30	100	Hard ground	B 0·5	49·6 48·2	
35	,,	10.20 ,,	60 34 20	8 51 0	124	Hard ground	B 0·5	49·1 47·8	
36	,,	11.0 ,,	60 31 45	8 47 45	129	Hard ground	B 0·5	48·4 48·5	
37	,,	11.41 ,,	60 29 0	8 43 45	175	Hard ground	B 0·5	47·8 46·8	
38	,,	0.25 P.M.	60 25 45	8 41 0	220	Sand and shells	B 0·5	47·4 46·8	

No. of sounding.	Date.	Hour.	Position.		Depth in fathoms.	Nature of bottom.	Bottom temperature.		Remarks.
			Lat. N.	Long. W.			No. of therm.	Result.	
			° ′ ″	° ′ ″				°	
39	Aug. 8th	1.15 P.M.	60 23 0	8 38 15	256	Sand and gravel	B 0·5	46·5 45·5	
40	„	2.5 „	60 20 30	8 36 0	267	Sand and gravel	B 0·5	47·2 46·8	
41	„	3.0 „	60 17 15	8 32 0	423	Stones	B 0·5	42·5 41·6	Serial temperatures. See Table II, No. 12.
42	„	4.35 „	60 19 20	8 27 15	305	Gravel	B 0·5	43·5 41·0	
43	„	5.20 „	60 21 0	8 23 30	280	Stones	B 0·5	37·5 37·0	
44	„	5.50 „	60 20 15	8 25 30	285	Gravel	B 0·5	35·8 35·5	
45	„	6.45 „	60 22 40	8 21 0	327	Stones	B 0·5	32·0 31·5	Serial temperatures and trawling station. See Table II, No. 11.
46	Aug. 9th	7.45 A.M.	60 31 15	8 14 0	430	Mud	B 0·5	30·5 30·0	Serial temperatures. See Table II, No. 9.
47	„	10.0 „	60 28 15	8 15 30	407	Mud	B 39,973	30·8 35·5	Result shown by No. 39,973. Thermometer rejected.
48	„	11.0 „	60 25 0	8 14 0	385	Mud	B 94	30·5 32·5	
49	„	Noon.	60 21 35	8 13 0	299	Sand and stones	B 94	34·2 34·1	
50	„	0.40 P.M.	60 19 30	8 12 0	268	Sand and gravel	B 94	44·5 44·8	
51	„	1.20 „	60 17 0	8 10 45	285	Sand and gravel	B 94	43·0 43·8	

No. of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
			° ′ ″	° ′ ″				°	
52	Aug. 9th	2.2 P.M.	60 14 45	8 9 45	306	Stones	B 94	43·8 44·2	
53	,,	3.0 ,,	60 12 0	8 7 50	353	Hard ground	B 94	44·5 45·0	
54	,,	4.0 ,,	60 8 25	8 5 30	458	Globigerina ooze	B 94	42·7 42·8	Serial temperatures. See Table II, No. 10.
55	,,	6.15 ,,	60 11 45	8 15 0	433	Hard ground	B 94	43·5 43·5	Trawling station.
56	Aug. 10th	6.0 A.M.	60 20 15	8 8 0	285	Stones	B 94	40·8 40·8	
57	,,	7.0 ,,	60 23 0	8 6 0	390	Stone	B 94	31·0 31·0	
58	Aug. 16th	9.0 ,,	59 39 0	6 43 0	435	Sand	B 94	47·5 49·2	Serial temperatures. See Table II, No. 1.
59	,,	11.0 ,,	59 41 15	6 38 15	242	Gravel	B XXIII	49·2 50·0	
60	,,	Noon	59 43 15	6 34 0	162	Sand and shells	B XXIII	49·4 50·2	
61	,,	0.40 P.M.	59 45 30	6 30 0	120	Sand and gravel	B XXIII	49·5 50·6	
62	,,	1.25 ,,	59 47 30	6 26 0	163	Mud, sand, and shells	B XXIII	49·2 50·0	
63	,,	2.11 ,,	59 49 30	6 21 30	222	Sand and gravel	B XXIII	49·2 50·0	
64	,,	3.0 ,,	59 51 45	6 17 45	240	Sand and gravel	B XXIII	47·7 48·7	

No of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
			° ′ ″	° ′ ″					
65	Aug. 16th	3. 50 P.M.	59 53 45	6 13 0	285	Sand and gravel	B XXIII	33·0 34·0	
66	,,	4. 45 ,,	59 56 15	6 8 0	313	Sand and gravel	B XXIII	33·5 32·5	Serial temperatures. See Table II, No. 2.
67	Aug. 17th	5. 15 A.M.	60 7 40	6 44 0	630	Mud, sand, and stones	B XXIII	30·4 30·8	Serial temperatures. See Table II, No. 3.
68	,,	7. 30 ,,	60 5 0	6 49 30	455	Sand and gravel	B XXIII	30·3 30·5	
69	,,	8. 30 ,,	60 3 0	6 54 0	359	Sand and gravel	B XXIII	30·5 30·0	
70	,,	9. 25 ,,	60 1 0	6 58 30	260	Sand and gravel	B XXIII	46·8 47·0	Serial temperatures. See Table II, No. 4.
71	,,	11. 0 ,,	59 58 45	7 3 0	262	Sand and gravel	B XXIII	47·5 47·5	
72	,,	Noon	59 56 20	7 8 0	330	Sand and gravel	B XXIII	47·0 47·4	
73	,,	0. 45 P.M.	59 54 10	7 12 50	409	Ooze	B XXIII	47·0 47·0	Serial temperatures. See Table II, No. 5.
74	,,	3. 5 ,,	60 0 45	7 16 0	246	Ooze	B XXIII	46·5 46·5	
75	,,	4. 5 ,,	60 3 10	7 11 30	295	Sand and stone	B XXIII	38·7 38·5	
76	,,	5. 0 ,,	60 5 30	7 8 0	375	Sand and stone	B XXIII	30·5 30·0	
77	,,	6. 20 ,,	60 9 0	7 16 30	466	Stones	B XXIII	30·0 29·5	Dredging station.

No. of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
			° ′ ″	° ′ ″				°	
78	Aug. 18th	7.45 A.M.	60 7 0	7 16 0	285	Sand and stones	B XXIII	47·0 46·0	
79	„	8.35 „	60 6 0	7 19 0	259	Sand and gravel	B XXIII	47·4 47·8	
80	„	9.25 „	60 5 0	7 23 45	263	Gravel	B XXIII	47·5 48·0	
81	„	10·35 „	60 9 0	7 26 0	270	Gravel	B XXIII	47·2 47·5	
82	„	Noon	60 9 40	7 32 0	299	Gravel	B XXIII	47·0 47·5	
83	„	0.40 P.M.	60 12 40	7 26 0	359	Stones	B XXIII	31·8 30·5	
84	„	1.30 „	60 11 50	7 28 0	305	Sand and gravel	B XXIII	46·0 46·5	
85	„	2.8 „	60 11 10	7 30 0	285	Sand	B XXIII	46·4 46·5	
86	„	3.6 „	60 10 0	7 33 45	315	Stones	B XXIII	46·7 46·9	
87	„	4.40 „	60 12 0	7 42 30	319	Stones	B XXIII	31·2 30·5	
88	„	5.21 „	60 11 10	7 44 50	315	Stones	B XXIII	32·0 31·0	
89	„	6.0 „	60 10 30	7 47 15	365	Sand and stones	B XXIII	30·5 32·7	
90	„	6.40 „	60 9 20	7 50 0	315	Sand and stones	B XXIII	33·0 31·5	

No. of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Results.	Remarks.
			° ′ ″	° ′ ″				°	
91	Aug. 18th	7.15 P.M.	60 8 30	7 52 0	336	Sand and stones	B XXIII	31·8 31·0	
92	”	8.0 ”	60 8 0	7 54 0	370	Gravel	B XXIII	35·0 33·2	
93	Aug. 19th	4.30 A.M.	60 2 0	8 11 0	450	Mud	B XXIII	45·0 44·5	Serial temperatures. See Table II, No. 6. Lost B. thermometer.
94	”	6.35 ”	60 4 0	8 6 0	450	Sand	0·5 XXIII	41·7 42·0	
95	”	7.40 ”	60 6 0	8 1 30	455	Mud	0·5 XXIII	39·0 39·0	Temperature at 435 fathoms, 44°·5 by 0·6 thermometer.
96	”	8.40 ”	60 7 30	7 57 0	465	Gravel	0·5 XXIII	38·0 38·0	Temperature at 465 fathoms. 41°·0 by 0·6 thermometer.
97	”	10.14 ”	60 12 20	7 44 0	328	Mud and stones	0·5 XXIII	30·5 30·3	Serial temperatures. See Table II, No. 7.
98	”	0.45 P.M.	60 15 20	7 30 0	396	Gravel	0·5 XXIII	30·2 30·2	Serial temperatures. See Table II, No. 8.
99	”	3.30 ”	60 18 0	7 38 0	440	Gravel	0·5 XXIII	30·0 30·0	
100	”	5.0 ”	60 19 27	7 50 15	437	Mud	0·5 XXIII	29·8 30·0	
101	”	6.0 ”	60 17 30	7 54 30	380	Sand and stones	0·5 XXIII	30·0 30·2	
102	”	7.0 ”	60 15 0	7 59 0	289	Gravel	0·5 XXIII	31·8 31·0	
103	”	7.40 ”	60 13 35	8 2 0	276	Sand and gravel	0·5 XXIII	32·0 31·2	

No. of sounding.	Date.	Hour.	Position. Lat. N.	Position. Long. W.	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
			° ′ ″	° ′ ″				°	
104	Aug. 21st	7.45 A.M.	60 21 0	7 4 0	432	Hard ground	0·5 XXIII	30·7 30·5	
105	"	9.20 "	60 19 0	7 10 0	585	Hard ground	0·5 XXIII	30·5 29·9	Dredging station.
106	Aug. 22nd	4.40 "	60 18 0	6 15 0	640	Mud	0·5 XXIII	30·0 30 0	Trawling station; two hauls.
107	Aug. 23rd	4.30 "	60 5 0	6 21 0	608	Mud	0·5 XXIII	30 0 30·0	Trawling station.
108	"	0.30 P.M.	59 59 45	6 19 0	235	Gravel	0·5 XXIII	46·5 50·0	
109	"	1.20 "	59 57 50	6 23 0	195	Gravel	0·5 XXIII	47·0 49·8	
110	"	2.15 "	59 55 0	6 28 0	187	Gravel	0·5 XXIII	47·2 49·0	
111	"	3.10 "	59 52 30	6 34 0	350	Hard ground	0·5 XXIII	47·2 49·0	
112	"	4.5 "	59 50 10	6 37 0	474	Gravel	0·5 XXIII	47·0 47·5	
113	"	5.30 "	59 54 40	6 41 0	306	Gravel	0·5 XXIII	48·5 49·0	
114	"	6.20 "	59 57 30	6 38 45	175	Gravel	0·5 XXIII	48·0 48·0	
115	"	7.0 "	60 0 0	6 37 30	187	Gravel	0·5 XXIII	48·0 48·0	
116	"	7.45 "	60 2 30	6 35 0	250	Gravel	0·5 XXIII	46·5 47·0	
117	"	8.20 "	60 4 20	6 31 0	424	Gravel	XXIII	29·6	

No. of sounding.	Date.	Hour.	Position. Lat. N. ° ′ ″	Position. Long. W. ° ′ ″	Depth in fathoms.	Nature of bottom.	Bottom temperature. No. of therm.	Bottom temperature. Result.	Remarks.
118	Aug. 24th	5.0 A.M.	59 40 0	7 21 0	516	Mud	0·5 XXIII	46°·0 46·5	Trawling station.
119	Aug. 28th	6.0 „	59 21 30	7 4 0	415	Mud	0·5 XXIII	46·9 46·9	
120	„	8.0 „	59 29 30	7 13 0	555	Ooze	0·5 XXIII	45·5 45·5	Trawling and dredging station.
121	Aug. 29th	5.0 „	59 58 15	7 28 45	305	Mud	0·5 XXIII	47·0 47·3	
122	„	5.55 „	59 59 50	7 25 30	310	Mud	0·5 XXIII	47·4 47·4	
123	„	6.55 „	60 1 50	7 20 45	262	Gravel	0·5 XXIII	45·5 45·5	
124	„	7.45 „	60 3 45	7 25 30	258	Gravel	0·5 XXIII	44·5 44·5	Temperature at 240 fathoms, 47° by 0·6 thermometer.
125	„	8.40 „	60 5 30	7 29 30	258	Mud and gravel	0·5 XXIII	44 0 43·8	Temperature at 240 fathoms, 46°·5 by 0·6 thermometer.
126	„	9.35 „	60 7 0	7 33 45	260	Gravel	0·5 XXIII	44·9 45·6	Temperature at 240 fathoms, 46°·9 by 0·6 thermometer.
127	„	10.28 „	60 8 20	7 38 45	278	Gravel	0·5 XXIII	44·6 44·4	Temperature at 260 fathoms, 46°·2 by 0·6 thermometer.
128	„	11.26 „	60 10 30	7 43 0	285	Sand and gravel	0·5 XXIII	44·7 44·5	Temperature at 265 fathoms, 48° by 0·6 thermometer.
129	„	0.25 P.M.	60 12 20	7 48 0	335	Gravel	0·5 XXIII	41·2 41·2	Temperature at 315 fathoms, 43°·5 by 0·6 thermometer.
130	„	1.25 „	60 13 50	7 52 0	322	Sand	0·5 XXIII	31·0 31·0	

No. of sounding.	Date.	Hour.	Position.		Depth in fathoms.	Nature of bottom.	Bottom temperature.		Remarks.
			Lat. N.	Long. W.			No. of therm.	Result.	
			° ′ ″	° ′ ″				°	
131	Aug. 29th	2.20 P.M.	60 12 30	7 55 15	325	Sand	0·5 XXIII	31·0 30·2	Temperature at 305 fathoms, 33·0 by 39,973 thermometer.
132	”	4.5 ”	60 8 0	7 44 20	319	Mud	0·5 XXIII	46·0 46·0	Temperature at 300 fathoms, 50° by 39,973 thermometer.
133	”	5.10 ”	60 10 30	7 39 0	305	Gravel	0·5 XXIII	42·0 42·0	
134	Aug. 30th	5.45 A.M.	60 31 0	7 34 0	580	Mud	0·5 XXIII	31·0 31·0	Trawling station; two hauls.
135	Aug. 31st	5.0 ”	59 51 20	8 18 0	570	Ooze	0·5 XXIII	45·7 45·7	Trawling and dredging stations.

Soundings obtained in North Atlantic, North-West of Ireland.

No. of sounding.	Date.	Hour.	Lat. N.	Long. W.	Depth in fathoms.	Nature of bottom.	No. of therm.	Result.	Remarks.
1	Sept. 13th	Noon	55 37 0	11 21 0	1360	Ooze	0·1 0·5	37·5 36·8	Temperature at 800 fathoms, 39·9; at 500 fathoms, 46°·6.
2	”	2.30 P.M.	55 37 0	11 16 0	1345	Ooze	..	..	

Table II.—Serial Temperatures obtained in Faeroe Channel by H.M.S. "Triton," August, 1882.

No. 1. No. of sounding 58. Section A. Warm area.
Lat. 59° 39′ 0″ N. Long. 6° 43′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 1.
Surface	..	57° 0′	57°·0
10	0·5	55 5	55·9
20	X	55 0	55·3
30	0·6	55 0	54·8
40	B	54 2	53·5
50	XXIII	50 0	50·1
	XXIII bis	50 0	
100	I	49 8	50·1
150	94	50 5	50·1
200	A 11	46 0	50·1
250	0·6	50 5	50·0
300	X	48 8	49·4
350	83	55 0	48·6
400	B	47 5	47·9
435	B	47 5	47·5
	94	49 2	

No. 2. No. of sounding 66. Section A. Cold area.
Lat. 59° 56′ 15″ N. Long. 6° 8′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 1.
Surface	..	56° 4′	56°·4
10	B	54 0	54·1
	41,054	50 0	
	41,049	48 0	
20	B	53 0	52·5
	41,054	50 5	
	41,051	48 0	
30	B	51 5	51·5
	41,051	49 0	
40	B	51 2	50·9
50	A 19	50 8	50·4
	41,049	50 0	
100	I	49 2	49·8
	41,054	50 1	
150	X	46 8	49·4
	41,051	52 1	
200	B	47 0	47·3
	0·5	46 8	
220	41,049	48 8	45·8
240	41,054	43 5	43·5
260	41,051	37 5	37·8
280	39,978	41 0	35·5
300	B	34 2	34·0
313	B	33 5	33·0
	XXIII	32 5	

No. 3. No. of sounding 67. Section B. Cold area.
Lat. 60° 7′ 40″ N. Long. 6° 44′ 00″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 2.
Surface	..	54° 5′	54°·5
10	0·6	53 0	53·0
20	0·1	51 5	51·3
30	0·5	49 8	50·1
40	XXIII	49 4	49·4
50	B	49 2	49·0
	X	48 8	
100	A 19	48 8	48·7
150	41,049	48 2	48·1
200	41,054	47 4	47·5
220	0·6	46 0	46·0
240	0·1	43 5	43·7
260	0·5	41 8	41·7
280	XIII	40 0	39·7
300	B	37 6	37·7
630	B	30 4	30·6
	XXIII	30 8	

No. 4. No. of sounding 70. Section B, on the ridge.
Lat. 60° 1′ 0″ N. Long. 6° 58′ 30″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 1.
Surface	..	55°·0	55°·0
10	0·1	54·0	53·9
20	0·5	53·0	52·8
30	XXIII	51·8	51·7
40	B	51·5	50·6
50	41,049	49·2	49·5
100	41,054	47·5	48·6
150	0·6	48·5	48·4
180	0·1	47·8	48·0
200	0·5	47·3	47·8
220	XXIII	47·8	47·6
240	B	47·2	47·3
260	XXIII	47·0	47·0
	B	46·8	

No. 5. No. of sounding 73. Section B. Warm area.
Lat. 59° 54′ 10″ N. Long. 7° 12′ 50″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 2.
Surface	..	55°·5	55°·5
10	0·1	54·0	54·5
20	0·5	53·0	53·4
30	XXIII	53·0	52·3
40	B	51·0	51·2
50	41,049	50·8	50·2
100	41,054	48·8	48·8
150	0·6	48·8	48·5
200	0·1	48·1	48·2
250	0·5	47·2	47·9
300	XXIII	48·2	47·6
350	B	47·2	47·3
400	B	47·0	47·0
	XXIII	47·0	

No. 6. No. of sounding 93. Section C. Warm area.
Lat. 60° 2′ 0″ N. Long. 8° 11′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 3.
Surface	..	54°·6	54°·6
10	0·6	53·0	53·1
20	0·1	53·0	52·2
30	0·5	51·2	51·3
40	XXIII	50·8	50·5
50	A 19	50·2	50·1
100	41,049	48·8	48·8
150	41,054	47·5	48·2
200	0·6	49·0	48·0
250	0·1	47·0	48·0
300	0·5	46·5	47·6
350	XXIII	48·0	46·9
400	B	lost	45·9
450	B	45·0	44·7
	XXIII	44·5	

No. 7. No. of sounding 97. Section C, on the ridge.
Lat. 60° 12′ 20″ N. Long. 7° 44′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 3.
Surface	..	55°·4	55°·4
10	41,054	53·5	53·6
20	XI	52·5	52·3
30	X	50·0	51·0
40	0·1	49·6	50·0
50	0·6	49·7	49·0
	A 25	48·2	
100	0·5	48·0	48·4
	83	47·5	
150	XXIII	48·0	48·4
200	41,054	49·0	48·4
220	0·6	49·0	48·4
240	0·1	47·5	48·2
260	X	48·0	47·8
280	0·5	46·8	46·4
300	XXIII	42·0	41·8
328	0·5	30·5	30·4
	XXIII	30·3	

No. 8. No. of sounding 98. Section C. Cold area.
Lat. 60° 15′ 20″ N. Long. 7° 30′ 0″ W.

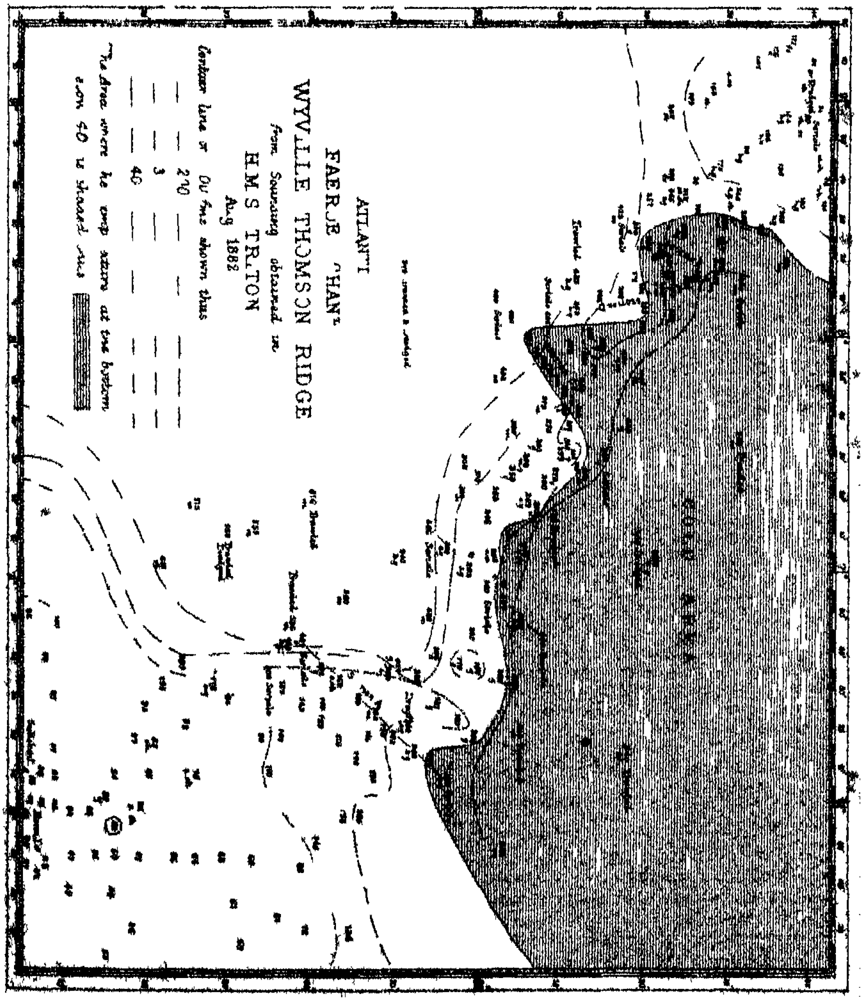
Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 3.
Surface	..	55°·8	55°·8
10	X	52·5	52·3
20	41,051	51·0	51·0
30	A 8	49·2	50·0
40	41,054	49·0	49·1
50	41,051	49·0	48·7
	A 18	49·0	
100	A 8	46·0	48·0
	A 25	48·0	
150	41,054	50·2	47·7
	0·1	47·2	
200	XI	47·5	47·5
220	X	48·8	47·4
240	0·1	47·0	47·3
260	0·6	47·5	47·2
280	0·5	47·2	47·0
300	XXIII	47·0	46·9
320	0·6	39·0	39·0
340	0·5	31·5	31·9
360	XXIII	30·5	30·5
396	0·5	30·2	30·2
	XXIII	30·2	

No. 9. No. of sounding 46. Sections D and E. Cold area.
Lat. 60° 31′ 15″ N. Long. 8° 14′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 4.
Surface	..	55°·0	55°·0
50	A 16	52 ·2	52 ·0
100	A 25	49 ·0	49 ·2
150	A 18	48 ·8	49 ·2
200	A 19	49 ·2	49 ·2
	41,051	50 ·5	
	A 18	51 ·0	
220	A 11	51 ·0	49 ·2
	41,054	52 ·0	
	44,565	53 ·0	
240	VIII	52 ·5	49 ·2
	44,558	48 ·0	
	41,051	49 ·2	
260	I.V	52 ·5	45 ·4
	44,565	51 ·5	
	41,054	45 ·0	
280	0 ·5	30 ·2	39 ·0
	39,973	44 ·0	
	39,973	39 ·5	
300	B	33 ·2	32 ·4
	B bis	32 ·0	
	B bis	36 ·0	
430	B	30 ·5	30 ·2
	0 ·5	30 ·0	

No. 10. No. of sounding 54. Section D. Warm area.
Lat. 60° 8′ 25″ N. Long. 8° 5′ 30″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 4.
Surface	..	55°·2	55°·2
50	0 ·5	49 ·0	49 ·0
100	I	48 ·8	48 ·8
150	83	48 ·5	48 ·8
200	10	48 ·8	48 ·8
250	XXIII	48 ·8	48 ·8
300	0 ·6	49 ·0	48 ·8
350	94	47 ·8	48 ·0
400	B	43 ·5	43 ·5
458	B	42 ·7	42 ·8
	94	42 ·8	



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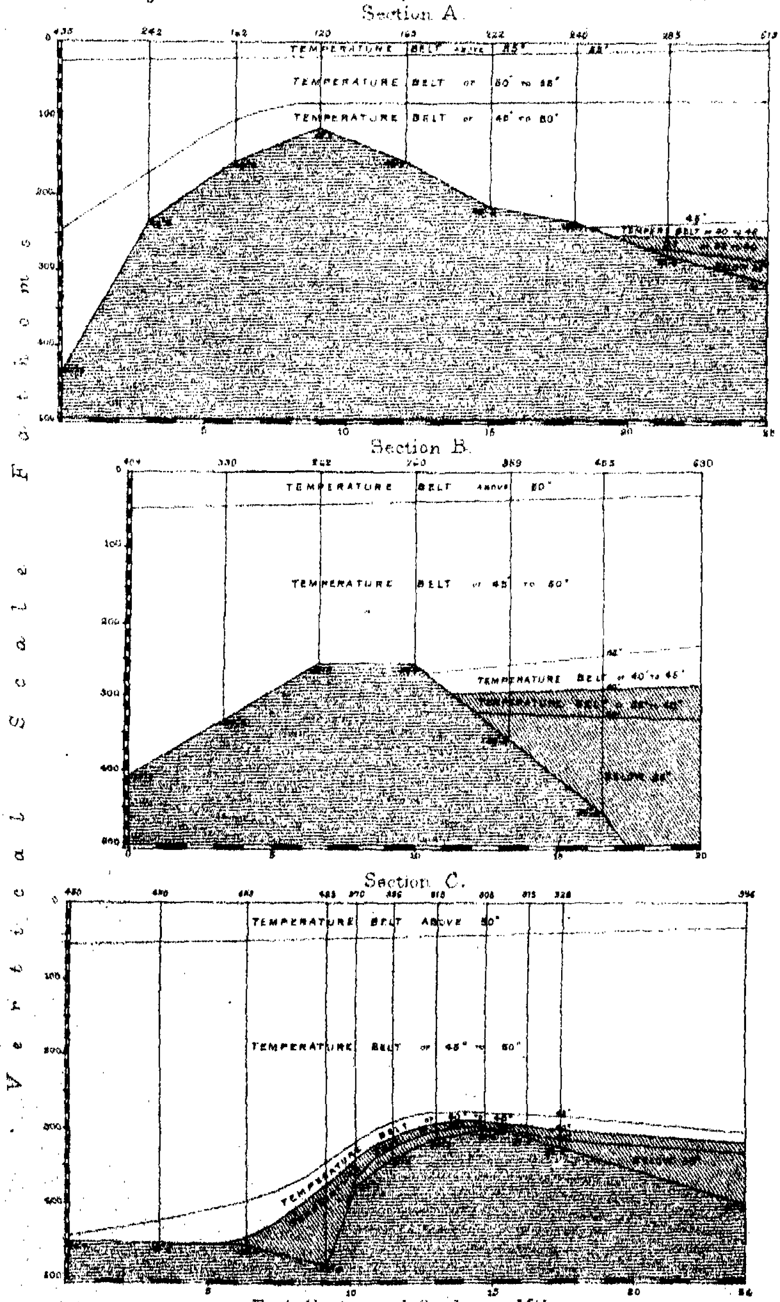
SECTIONS OF WYVILLE THOMSON RIDGE, FAEROE CHANNEL

Obtained by H. M. S. TRITON, August 1882.

showing the distribution of the temperature from the Surface to the Bottom



Each Horizontal Scale — Miles.

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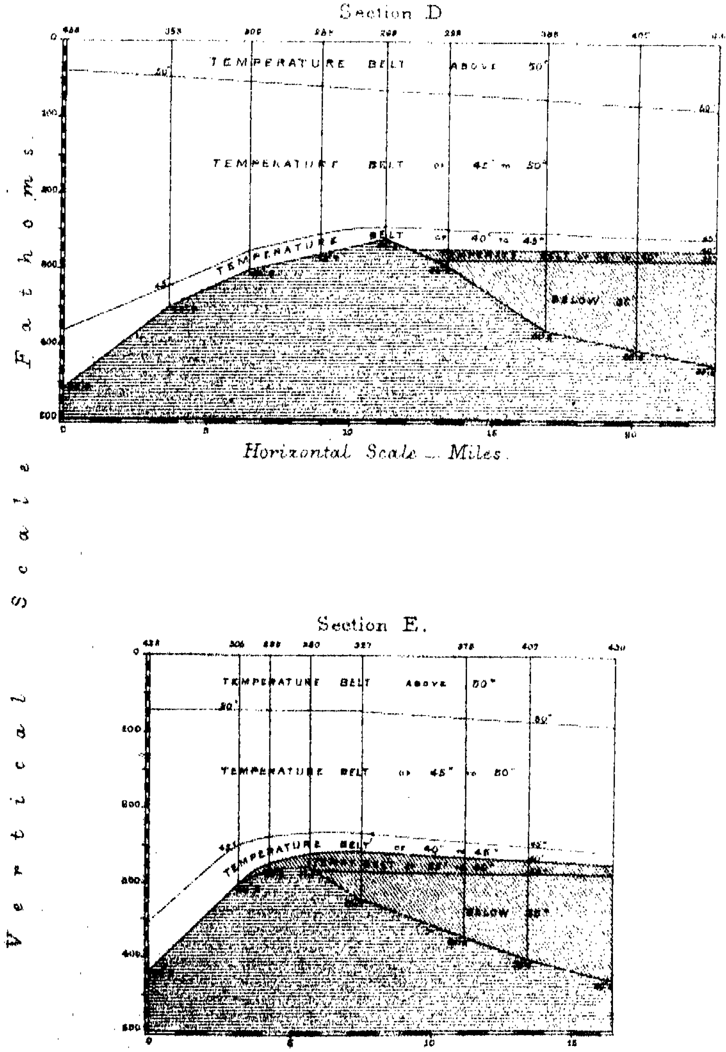
SECTIONS OF WYVILLE THOMSON RIDGE, FAEROE CHANNEL

Obtained by H. M. S. TRITON, *August* 1882

showing the distribution of the temperature from the Surface to the Bottom



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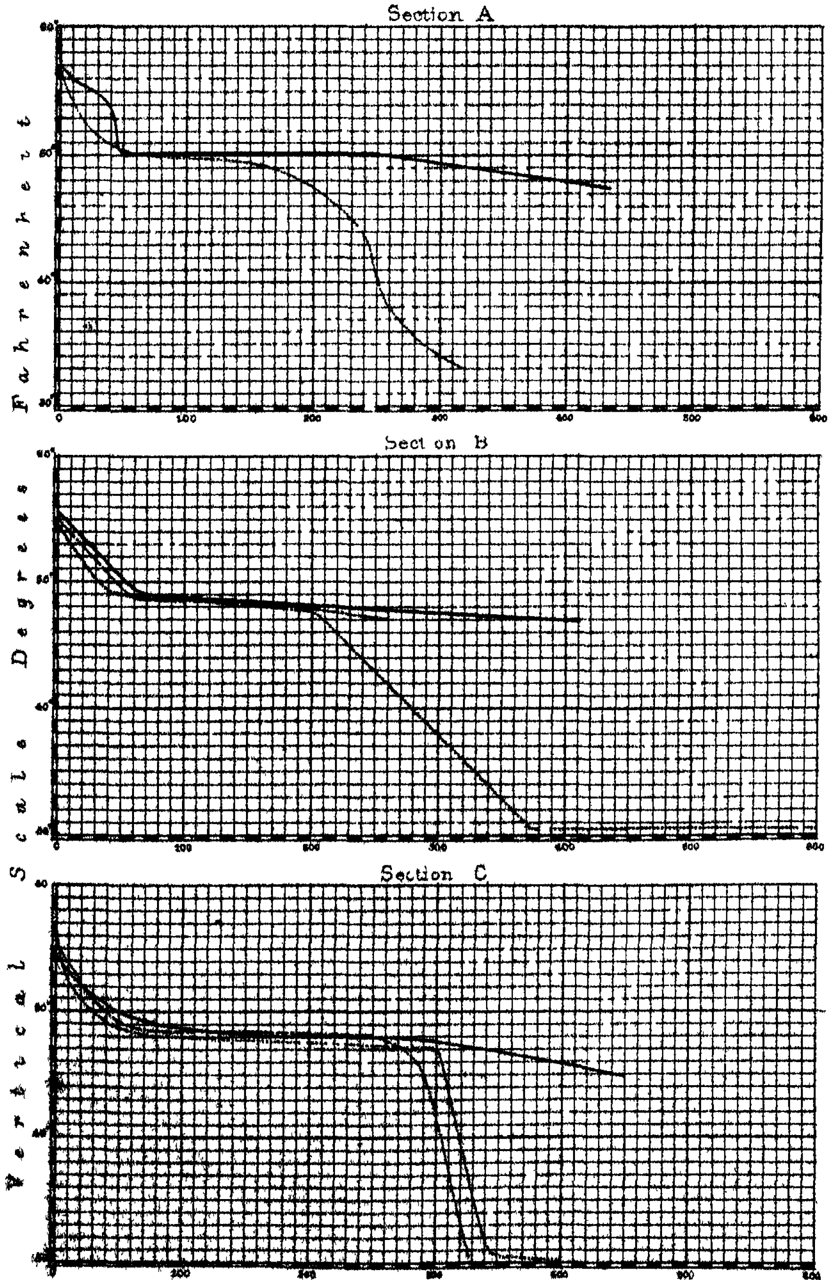
H M S TRITON SERIAL TEMPERATURE CURVES
WYVILLE THOMSON RIDGE FAEROE CHANNEL



Horizontal Scale Fathoms
Warm Area ——— Cold Area .. On Ridge —.— —

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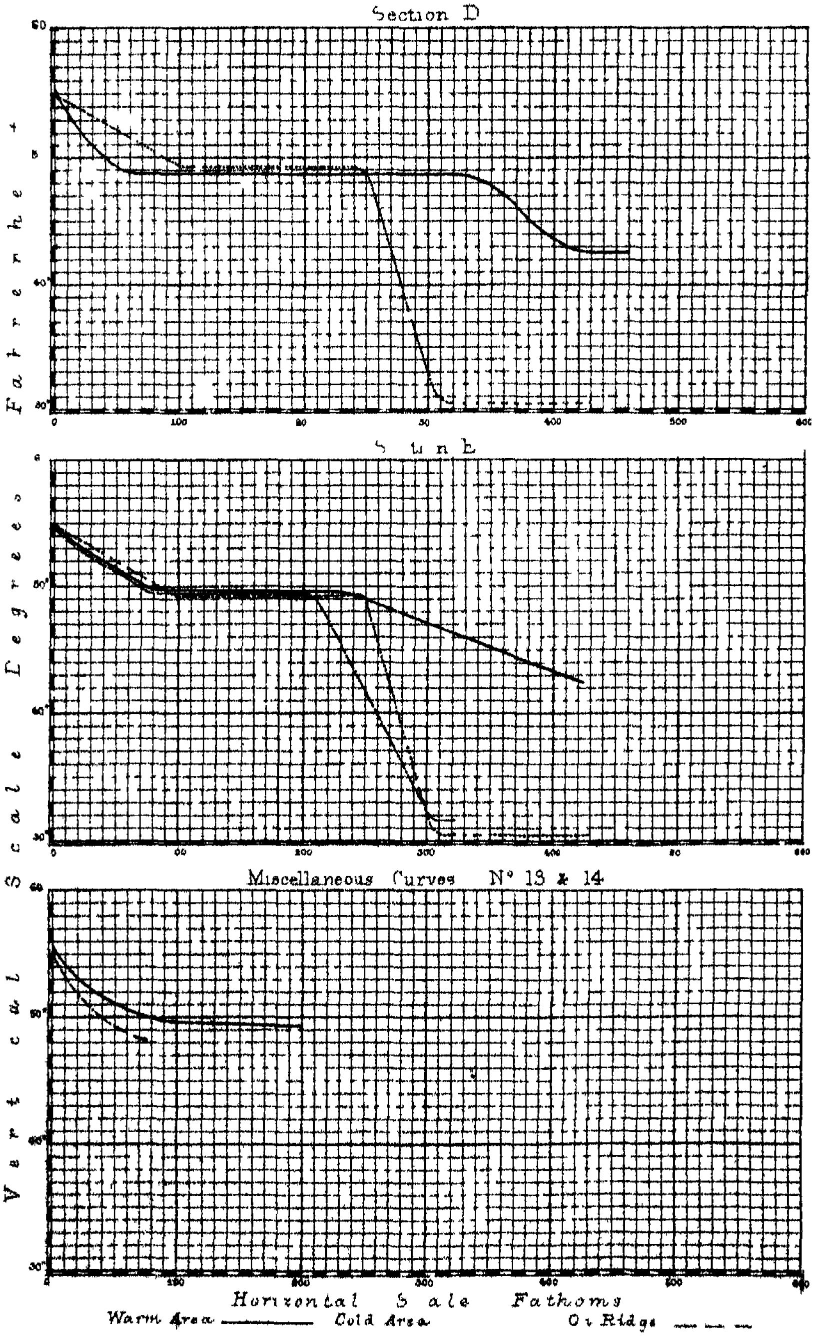
H M S TRITON SERIAL TEMPERATURE CURVES
WYVILLE THOMSON RIDGE FAEROE CHANNEL



No. 11. No. of sounding 45. Section E, on the ridge.
Lat. 60° 22′ 40″ N. Long. 8° 21′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 5.
Surface	..	55°·0	55°·0
50	0·1	51·0	51·0
100	X	50·0	49·5
150	LV	48·8	49·5
200	VIII	50·0	49·5
220	80	47·5	47·5
240	I	41·8	44·3
260	XI	43·0	40·5
280	0·5	27·0	36·2
300	B	31·2	32·2
327 {	B	32·0	} 31·7
	0·5	31·5	

No. 12. No. of sounding 41. Section E. Warm area.
Lat. 60° 17′ 15″ N. Long. 8° 32′ 0″ W.

Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 5.
Surface	...	55°·0	55°·0
50	I	51·2	51·0
100	XI	49·5	49·5
150	III	49·5	49·5
200	VIII	49·5	49·5
250	LV	49·2	49·0
300	0·5	46·0	47·0
350	B	45·2	45·2
428 {	B	42·5	} 42·0
	0·5	41·6	

No. 13. No. of sounding 1. Section —. Warm area. Lat. 59° 34′ 30″ N. Long. 6° 36′ 30″ W.			
Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 6.
Surface	LV	55°·5	55°·5
20	XI	53·0	53·0
40	VIII	51·5	51·5
60	83	50·8	50·6
80	0·6	50·0	50·0
100	A 8	Mercury broken	49·8
120	A 11	49·9	49·7
140	X	49·5	49·5
160	III	48·2	49·4
180	0·5	47·2	49·3
200	B	49·8	49·3
	0·5	49·3	

No. 14. No. of sounding 33. Section —. Warm area. Lat. 60° 39′ 30″ N. Long. 8′ 55′ 45″ W.			
Depth in fathoms.	Distinguishing mark of thermometer.	Reading.	Temperature by curve, diagram No. 6.
Surface	..	55°·0	55°·0
20	VIII	52·0	51·9
40	0·5	50·0	49·8
60	B	49·8	48·8
80	B	47·8	48·0
	0·5	48·0	

III. "Preliminary Note on the Innervation of the Mammalian Heart." By L. C. Wooldridge, D.Sc., M.B., George Henry Lewes Student. Communicated by Dr. M. Foster, Sec. R.S. Received April 23, 1883.

The research was carried out in the Physiological Institute at Leipzig. The immediate object was to determine the function of nerves which are to be seen on the surface of the ventricles of the hearts of mammals. It was important to know their functions on the following grounds:—

In the frog, stimulation of the sinus produces stillstand of the heart. The inhibitory fibres have at any rate here a provisional ending.

On the other hand the result of stimulation of the ventricle may be regarded as a form of acceleration.

In the dog, the vagus and accelerans nerves act on different mechanisms (Baxt).

Having regard to these facts the possibility of the ventricle nerves being accelerator presented itself.

To know the function of these nerves was not only interesting, *per se*, but also as forming an appropriate introduction to the nearer investigation of the nervous mechanism of the mammalian heart.

The following observations on the accelerans will be first recorded:—

Hitherto in the dog, the experiments on the accelerans nerve have been carried out almost exclusively with that of the right side. It was more convenient in the present case to work with the left nerve (Ansa Vieusseni). The author has observed that in many cases stimulation of the left accelerans is without any influence on the rhythm of the heart; and that this was not due to accident, such as lowering of the temperature, was shown by the control stimulation of the right accelerans. This fact is not without importance for the remainder of the research, as will be seen.

Minimal electrical stimulation of the vagus overcomes completely the action of the accelerans, but the accelerans overcomes the normal slight tonic action of the vagus (Baxt). In an experiment of the author there existed, owing to stimulation of the medulla, a most powerful tonic vagus action. Thus—

Average of pulse beats before division of vagi....	8
" " after " "	18

Yet stimulation of the accelerans overcame this and produced marked quickening. If during the stimulation of the accelerans a very small cardiac branch of vagus (in the thorax) were stimulated, it exerted its inhibitory influence, and overcame the accelerans, though it did not depress the pulse to the same degree that the tonic vagus action did. With our present knowledge, this experiment points to a difference between the stimulation of a nerve from its centre, and the electrical stimulation of its trunk.

The ventricular nerves are very numerous, but require in the dog the use of special methods, in order to be seen well; the author recommends strong carbolic acid for this purpose. These nerves form at any rate the greater part of the nervous connexion between the auricle and ventricle. They cannot be adequately stimulated after they have passed on to the ventricle, since the stimulus affects the heart itself too. This is more particularly the case for electrical stimulation. As

is well known, very slight stimulation with a Faradaic current destroys the activity of the ventricle by bringing about a peculiar condition of fibrillar contractions.

The author's procedure was as follows: The ventricular nerves pass on to the ventricle at definite points of the auricular ventricular boundary, where they are collected into larger trunks. He has observed that division of these trunks has no influence on the rhythm of the heart, nor does it in any way impair the action of the vagus or accelerans nerve. These ventricular nerves are therefore not essential to any of these processes.

The ventricular nerves are the continuation of certain definite cardiac nerves, which can be isolated in the thorax at a distance from the heart. When these cardiac nerves are stimulated, some of the ventricular nerves must be stimulated too. In particular, the majority of the nerves on the posterior surface of the heart, are derived from a trunk which arises either from the left vagus ganglion, or from some part of the Ansa Vieusseni. This nerve usually runs quite isolated to the heart. The result of stimulation of the peripheral end of this nerve are as follows:—

Out of 14 experiments, it exerted in 4 a vagus action, without any acceleration; in 2 an acceleratory action without any inhibitory; in 8 it had no influence on the rhythm. The nerve sometimes gives off obvious branches to the auricle; in some of the cases where no influence on the rhythm was produced, these had been cut away. Particular attention was given to this nerve, because it is easy to isolate, and because it certainly contains fibres which go on to the ventricle.

The author also stimulated the other cardiac branches, which are in obvious connexion with the ventricular nerves. Sometimes they produced inhibition, sometimes acceleration, but also in this case the division of the trunks which continue these nerves on to the ventricle did not produce any change in the result of their stimulation. The author from the above observations concludes that the ventricular nerves have no direct influence on the rhythm of the heart.

Stimulation of the central end of those cardiac nerves, which are continued on to the ventricle, is followed by marked reflex phenomena; and this fact, in conjunction with the negative result just recorded, leads the author to regard the ventricular nerves as being chiefly sensory, or more exactly, centripetal.

The reflex phenomena are, rise and fall of blood pressure, slowing and quickening of the pulse. On placing a small piece of blotting paper soaked in acetic acid on to the surface of the ventricle in the rabbit, a rise of blood pressure was observed; the acetic acid was moderately strong; a second application to the same part had no effect. On tearing through the nerve trunks from which the ven-

tricular nerves start, reflex movements of the animal (dog) were observed.

The reflex acceleration of the heart beat, to be obtained by central stimulation of the cardiac nerves, is marked, and is not due to change in blood pressure. The fact that sensory nerves go to the heart was shown long ago by Ludwig and Cyon's discovery of the depressor nerve in the rabbit.

In the dog a large nerve (or two smaller) runs from the left vagus ganglion and sometimes from the trunk, and ends chiefly between the coats of the aorta, giving occasionally a branch to the Arteria Pulmonalis. The peripheral stimulation of this nerve is without effect. The central stimulation produces slowing of the heart and fall of blood pressure; sometimes the slowing is followed by acceleration. The nerve is very sensitive to mechanical stimulation.

The extent and importance of the centripetal nerves which come from the heart and great vessels, is clearly shown in the author's experiments. Whether the ventricular nerves are solely centripetal or not has not been fully determined. It is rendered probable by the author's experiments, that both vagus and accelerans act on mechanisms in the auricles. In some cases the author has observed changes of blood pressure follow stimulation of the peripheral ends of nerves going direct to the heart, either without any change in the beat, or without a corresponding change. But his observations on this point are too few to draw definite conclusions. The mercurial manometer was used. The dogs were narcotized with opium, and then the brain divided through the pons, the object being to render the subsequent steps of opening the thorax painless, and still to preserve reflex actions.

IV. "Note on the Motor Roots of the Brachial Plexus, and on the Dilator Nerve of the Iris." By DAVID FERRIER, M.D., LL.D., F.R.S., Professor of Forensic Medicine in King's College. Received April 24, 1883.

In a communication to the Royal Society (published in the "Proc. Roy. Soc.," vol. 32, 1881) on the "Functional Relations of the Motor Roots of the Brachial and Lumbo-Sacral Plexuses," my colleague, Professor Gerald Yeo, and myself gave an account of the results of electrical stimulation of the several motor roots of the brachial and crural plexuses in the monkey. We there described the muscular actions of the upper extremity as resulting from stimulation of the first dorsal up to the fourth cervical nerve.

The careful dissections made at our request by Mr. W. Tyrell Brooks, Demonstrator in the Physiological Laboratory, King's College,

and a repetition of the stimulation experiments which I have made, have revealed an error in the enumeration of the roots of the brachial plexus, which, in common with Professor Yeo, I wish to correct. What we took for the first dorsal nerve has proved in reality to be the second dorsal. Hence the results of the experiments must be read as applying to the spinal nerves from the second dorsal to the fifth cervical respectively, instead of from the first dorsal to the fourth cervical, as stated in our paper.

The anterior division of the second dorsal nerve in the monkey, apparently invariably, gives a well developed communicating branch to the first dorsal, besides giving off the second intercostal nerve and a branch to the stellate or inferior cervical ganglion of the sympathetic.

The three branches, as seen in a dissection made for me by Mr. Brooks, seem pretty equal in size, and all come off from the main trunk together.

The brachial plexus in man is not usually, in text-books of anatomy, considered as deriving any of its component roots below the first dorsal. In "Quain's Anatomy" (9th ed., p. 619), however, a branch from the second to the first dorsal is given as a variety. On this subject Dr. D. J. Cunningham has published a note in the "Journal of Anatomy and Physiology," vol. xi, Part III, p. 539, 1877. Dr. Allen Thomson having mentioned to him that he had on one or two occasions seen such a communicating branch in man, he investigated the point, with the result of finding a communicating branch from the second to the first dorsal in twenty-seven out of thirty-seven dissections. Of the ten cases where it was not found, five were so complicated by previous interference in the dissecting-room or by pleuritic adhesions and thickenings, that they may be considered as doubtful. But, even including these, it appears that the second dorsal sends a communicating branch to the first in seventy-three per cent. of the cases. Hence it should be considered as more than a mere variety. If a perfect homology exists between the roots of the plexus in man and the monkey, the second dorsal root would be the one presiding over the intrinsic muscles of the hand. Presumably in those cases where it is not found, its functions are represented in the first dorsal.

Dilátor Nerve of the Iris.—Professor Yeo and I mentioned in our paper (*sup. cit.*) that in one case in which we directed special attention to the pupil, stimulation of the anterior roots from the first dorsal to the fourth cervical—in reality from the second dorsal to the fifth cervical—caused no change in the pupil, though the movements of the limb occurred with regularity.

I have since investigated this point during the course of another research on which I have been for some time engaged. I have

experimented on four monkeys. The animals were thoroughly narcotised with chloroform, and kept so during the whole course of the experiments. The posterior roots of the nerves under investigation were cut, and the anterior stimulated within the vertebral canal, with a weak induced current from the secondary coil (distant 20—15 centims.) of a Du Bois Reymond's magneto-electromotor and one Daniell. As in former experiments, a large flat electrode was placed on the sacrum as a neutral point, the exciting electrode being a hooked needle, by means of which the roots could be easily insulated and separately stimulated.

In the first experiment I failed to obtain dilatation of the pupil from stimulation of the spinal roots from the second dorsal up to the fourth cervical, though the functional activity of the roots was indicated by movements of the limb. In the second I exposed the dorsal roots from the eighth up to the third inclusive. Though different strengths of current were tried no change in the pupil occurred, unless when the current was so strong as to cause diffuse stimulation. In such case both pupils would occasionally become dilated, as under sensory stimulation in general. The functional activity of the roots under investigation was shown by contraction of the thoracic muscles on the side of stimulation.

In the third experiment, however, results were obtained of such definiteness and uniformity, as to indicate almost without further confirmation the origin of the dilator nerve of the iris.

In this experiment the spinal nerves were exposed from the sixth cervical to the eighth dorsal inclusive. The posterior roots were cut on the left side, and the anterior roots stimulated, while the eyes were carefully observed by two assistants—my pupils, Mr. Norvill and Mr. East. Dilatation of the left pupil occurred almost invariably on stimulation of the second dorsal root, whereas no change whatever could be perceived on stimulation of any of the other exposed roots. This was verified over and over again, and the several roots repeatedly compared with each other. The distance of the secondary coil in this experiment ranged from 20—18 centims.

Stronger currents not carefully insulated caused dilatation of both pupils wherever the stimulation was applied, an expression only of general sensory stimulation.

After death a careful dissection was made for me by Mr. Brooks, and the effective root, which was marked, proved to be the second dorsal. An examination with a lens showed that the fibres of the posterior root of this nerve had been completely severed.

The results of the third experiment were entirely confirmed by the fourth, which was carried out alike in every detail.

In this I exposed the spinal nerves from the seventh cervical to the fourth dorsal, and cut the posterior roots on the left side.

Here again with the utmost uniformity on each stimulation of the second dorsal the left pupil, and this one only, became widely dilated, whereas stimulation of the other roots was entirely negative in respect to the pupil.

I ascertained in this experiment that a strength of current which would suffice to excite the muscles of the limb or trunk to action, would frequently fail to cause any dilatation of the pupil when applied to the second dorsal. Somewhat stronger, but yet barely perceptible on the tongue, the current at once caused the pupil to dilate. Occasionally also if the second root had been stimulated repeatedly, the iris failed to respond, probably from mere exhaustion of the nerve.

Circumstances such as these would, I think, account for the absence of the pupil-reaction in my first experiment, and also in the experiment related by Professor Yeo and myself, where the second dorsal root was really under stimulation.

The general result of these experiments is to show that in the monkey, and presumably also in man, the dilator fibres of the iris, contained in the cervical sympathetic, are derived from the anterior root of the second dorsal nerve.

The Society adjourned over the Whitsuntide Recess to Thursday, May 24th.

Presents, April 5, 1883.

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May 24, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Function of the Sound-post and on the Proportional Thickness of the Strings of the Violin." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received May 21, 1883.

Sir John Herschel says: "It (the bridge) sets the wood of the upper face in a state of regular vibration, and this is communicated to the back through a peg set up in the middle of the fiddle, and through its sides, called the 'soul' of the fiddle, or its sounding-post."*

Savart says: "L'âme a pour usage de transmettre au fond les vibrations de la table . . . . son diamètre est determiné par la qualité du son qu'on veut avoir; il est maigre quand elle est trop mince, et sourd quand elle est trop grosse."†

Daguin, in his "Traité de Physique," devotes a whole page to the discussion of the functions of the sound-post. The most important sentences are the following:—" . . . . l'âme n'agit pas comme conducteur du son . . . . Il nous semble que l'on doit expliquer l'effet de l'âme de la manière qui suit. L'âme, ou les pressions extérieurs par lesquelles on la remplace, a pour effet de donner au pied du chevalet un point d'appui autour duquel il vibre en battant sur la table de son autre pied. Si l'un des pieds n'était appuyé sur un point fixe, il se releverait pendant que l'autre s'abaisserait, parceque les cordes n'agissent pas normalement à la table, puisque l'archet les ébranle très obliquement, ce qui entraîne le chevalet dans un mouvement transversal quand il n'a pas de point d'appui fixe. Lorsque l'archet est dirigé normalement aux tables, cet inconvénient n'existe plus, et l'âme n'est plus nécessaire."‡

Helmholtz says: "The vibrating strings of the violin, in the first place, agitate the bridge over which they are stretched. This stands on two feet over the most mobile part of the belly between the two

* "Encyclopædia Metropolitana," Article "Sound," p. 804.

† "Mémoire sur la Construction des Instruments à Cordes et à Archet," 8vo., Paris, 1819. Also Biot's "Report," "Ann. de Chimie," tome 12, pp. 225–255.

‡ "Traité de Physique, Acoustique," tome 1, p. 575.

'*f*' holes. One foot of the bridge rests upon a comparatively firm support, namely, the sound-post, which is a solid rod inserted between the two plates, back and belly, of the instrument. It is only the other leg which agitates the elastic wooden plates, and through them the included mass of air."*

The experiments† which follow have been made for the purpose of ascertaining whether it be any part of the function of the sound-post to convey vibrations to the back, or whether this post acts solely as a prop supporting the belly, so that its elasticity is not injured by the pressure from the strings, and also, as Daguin states, affords the firm basis which he considers necessary for one foot of the bridge.

Mr. Hill, and other practical men, maintain that the quality of the wood of which the sound-post is made affects the tone of the violin, as undoubtedly do very minute differences of position. If the quality of the wood is important we must admit that vibrations are conveyed by the post.

Whether or not the sound-post exercises the function of transmitting vibrations, it is obvious (1) that it performs the important duty of contributing to the support of the belly; (2) that the nodal arrangement of the belly, and also that of the back, are influenced by the pressure of the ends of the post against the upper and lower plates; (3), that Helmholtz is right, at least so far that the leg of the bridge under the 4th or G string has much more power than the other, in setting the belly into vibration.

The usual way of investigating vibrations by the scattering of sand over the surface of the agitated body is difficult of application to the violin, on account of the curved form of the upper and lower plates. I found a convenient method to be by the use of what I may call a touch-rod. It consists of a small round stick of straight-grained deal a few inches long; the forefinger is placed on one end, and the other end is put lightly in contact with the vibrating surface. The finger soon becomes very sensitive to small differences of agitation transmitted by the rod.

The experiments were made on a strongly made modern violin, and in some cases repeated on a fine violin by Stradiuarius in the possession of the writer.

The sand method, and also the touch-rod, showed that the position of maximum vibration of the belly is close to the foot of the bridge under the 4th or G string. The place of least vibration is exactly over the top of the sound-post behind the other foot of the bridge. The back is strongly agitated, the vibrations being least

* "Sensations of Tone," translated by Ellis, p. 137. In the 4th German edition this passage remains unaltered.

† I wish to express my indebtedness to Mr. A. J. Ellis, F.R.S., for some suggestions in connexion with these experiments.

powerfully felt where the sound-post rests, which is at nearly the thickest part of the back. These effects were very satisfactorily observed on a violoncello, where the phenomena are on a larger scale.

When the sound-post was removed from the violin, the large difference of the amount of vibration on the two sides of the belly was no longer present, the belly was about equally strongly agitated on both sides, making allowance for the string which was bowed. The tone became very poor and thin, as is well known to be the case when the sound-post is removed. The vibration of the back was now very feeble, as compared with its vibration when the sound-post was present, a circumstance in favour of the view that the sound-post conveys vibrations to the back.

A clamp of wood was prepared which could be so placed on the violin, as to connect by an arch of wood outside the violin the place of the belly behind the bridge where the top of the sound-post presses, with the place of the back where it rests. It was expected that the wooden arch would restore to some extent the connexion of belly and back which was broken by the removal of the post, and carry, though imperfectly, vibrations from the upper plate to the back.

When this clamp was put on, the poor and thin sound was altered to the fuller character of tone which belongs to the violin when the sound-post is in its place. On testing the condition of the back its normal state of vibration was found to be in a large degree restored. If, while the strings were being bowed, the clamp was suddenly removed, the tone at the same moment fell to its poor character, and the vibration of the back as instantly diminished.

It was further observed that if the upper part of the clamp pressed upon the belly without the lower part coming into contact with the back, the tone was altered in the same direction as when the sound-post was present, but it was not until the lower part of the clamp was in contact with the back that the normal character of the tone was fully restored. A similar effect to that resulting from the pressing of one end of the clamp only was produced by firmly placing one end of a wooden rod at this part of the belly. This effect may be due to the setting-up in the belly, by pressure at this part, of the peculiar nodal arrangement which the post produces when in its place.*

* According to Daguin some similar experiments were made by Savart, but I have failed to find them in those of his papers to which I have had access.

"On peut la (l'âme) mettre en dehors, en l'appuyant à une espèce d'arcade dont on colle les pieds de chaque côté du violin. . . . . On peut la remplacer par la pression d'un poids convenable appuyé sur la table supérieure." "Savart a conclu de là que l'âme a pour effet de rendre normales les vibrations de la table. . . ." "Traité de Physique," tome I, p. 575.

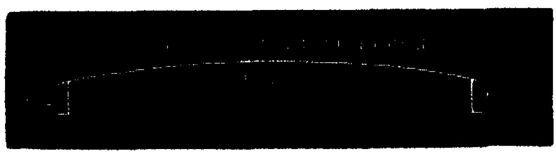
There could be no doubt that vibrations were carried by the clamp, for the lower end was powerfully agitated when the upper end rested upon the belly. If the sole function of the sound-post is to serve as a firm prop for the foot of the bridge, it should fulfil this condition most fully when placed under the foot of the bridge. In this position of the sound-post, however, as is well known, the tone is much injured.

In order to separate that part of the function of the sound-post which serves as a support from the further function it may possess as a transmitter of vibrations, it was desirable to introduce such alterations in the structure of the sound-post as would enable it to retain its supporting power, and yet greatly modify and, if possible, stop its power of transmitting vibrations. A sound-post was made in which about half an inch of the middle was cut out, and a piece of lead inserted, also a sound-post in which instead of lead sealing-wax was put in. The effect of these compound posts, which retained uninjured their prop power, was to modify greatly the quality of the tone, but not to diminish its quantity in any marked degree, a result in favour of the view that the character of the wood of which the post is made does influence the tone, and that vibration is transmitted by the post. As these compound posts could transmit vibrations freely, it was desirable to contrive a post which would not carry vibrations and yet form a firm prop. A post was made with a piece of hard India-rubber inserted in the middle, but this post was found by experiment with a tuning-fork to transmit vibrations to some extent. Other materials were tried without success. A post capped at each end with pieces of sheet vulcanized rubber stopped almost completely the sound of a tuning-fork when the foot of the fork rested on the rubber over one end of the post, while the other end equally protected with rubber rested on a body capable of reinforcing the sound of the fork. This rubber-capped post was firmly fixed in position in the violin, so that it would be able to support fairly well the belly and foot of the bridge, and yet not be able to carry vibrations; unfortunately it does not seem possible, from the nature of things, to have a *rigid* prop which does not transmit vibrations, but this post with thin sheet rubber at the ends firmly forced into position, must have been fairly efficient in its supporting power. The effect on the tone was about the same as when the sound-post was removed. When the wooden clamp was put on, then the normal tone returned, and the back vibrated strongly.

These experiments appear to show that the sound-post is more than a prop, and that besides its other functions, it does transmit vibrations to the back in addition to those which are conveyed through the sides.

Experiments with sand and the touch-rod appear to me to show that Helmholtz's statement is too absolute when he says "it is only

the other leg of the bridge which agitates the elastic wooden plates." Undoubtedly it is the 4th string foot of the bridge which is the more powerful in agitating the upper plate, but the other foot appears to me also to have an influence. When the post is placed exactly under the foot of the bridge, then the belly on this side is almost without vibration; if the post is absent, then this foot appears to agitate its own side of the belly as strongly as the other foot. As there is no post on the 4th string side of the fiddle, that foot stands in a position most favourable for setting up vibrations in the belly, being nearly halfway between the supports of the belly at the



tail and the neck end of the violin. The other side of the belly, on the 1st string side, where the other foot of the bridge rests, is divided into two parts by the damping effect of the end of the sound-post, namely, the part *a* and the part *b*. It is obvious that this foot

of the bridge is unfavourably placed for setting the part of the belly, *b*, into vibration, since it is so far from its central mobile part. On the other hand, its position is favourable for a portion of its energy of vibration to be transmitted through the post to the back.

Practically very small differences of position of the top of the post behind the foot of the bridge are found to alter largely the character of the tone of the fiddle, and in the case of fine instruments the setting of the post is an operation demanding much care and judgment. The explanation lies probably in the circumstance that a small difference in the position of the post will alter greatly the proportion of energy passing through the post to that which is absorbed into vibrations of this side of the belly. At the same time it must also alter slightly the nodal arrangement of the belly which must have an influence on the tone. If from the form of construction, or relative quality of the wood of the upper plate as compared with the under plate, the conditions of a violin are such that the highest quality of tone of which it is capable requires a relatively larger amplitude of vibration of the back, the position of the sound-post should be nearer

the bridge. In a contrary condition of things the sound-post should be farther from the bridge. The extreme range needed in different violins is about a quarter of an inch. At the same time any shift of the post must affect the relative mobility of the two sides of the belly.

If the sound-post transmits vibrations, these will be in addition to those received from the sides of the violin. It may be, therefore, that one condition which determines the best position of the post, is the degree in which from their form and material these fulfil this duty. All the sides must share in this duty, but the touch-rod shows that a large part of this action is borne by the parts of the sides which curve inwards under where the strings are bowed. It is in harmony with this view that Mr. Hill states, that if the inside blocks at the corners, which are put to strengthen these parts, extend in a small degree into these curved portions, the tone is injured.

The plane of the vibrations of the strings is that in which they are bowed, which is more or less oblique to the bridge. The vibrations may be considered divided into two sets at right angles to each other, *a* and *b*.

The touch-rod shows that these vibrations exist strongly in the upper part of the bridge. I venture to suggest that the use of the peculiar cutting of the bridge, which was finally fixed from trials by Stradiuarius, is to sift the vibrations communicated by the strings and to allow those only, or mainly, to pass to the feet which would be efficient in setting the body of the instrument into vibration, the other vibrations which would be injurious in tending to give a transverse rocking motion to the bridge being for the most part absorbed by the greater elasticity given to the upper part of the bridge by the cutting. Below the two large lateral cuts the touch-rod shows a very great falling off of the vibrations *b*. In the case of a violoncello these vibrations were also very greatly reduced below the side openings of the bridge.

The violin on which the experiments were made was without a bass bar, which is a piece of pine glued to the under side of the belly on

the 4th string side. This bar is regarded as strengthening the belly, and also enabling it to respond better to the lower notes. The touch-rod showed no difference in the general behaviour of this violin, from a fine one by Stradiuarius containing a bass bar.*

On the Proportional Thickness of the Strings.

As the lengths of the strings are the same, we have only the two conditions of weight and tension on which their pitch depends. It is obvious that for equal pressure on the feet of the bridge, as well as for more convenient fingering and bowing, the strings should be at the same tension. They should, therefore, differ in weight, so as to give fifths when brought to the same tension. The weights of the strings must be inversely as the squares of the number of vibrations, which, in the case of fifths, is as 3 to 2, namely, as 9 to 4. As the first three strings are of the same material, it is more convenient to take their diameters, which must be as 3 to 2, that is, each string in advancing from the 1st string must be half as thick again as the string next to it. In the case of the 4th string covered with wire, we must find the weight of the 3rd string of gut, and take a 4th string of which the weight is 9 to 4 for the 3rd string.

A good average thickness of 2nd (A) string is 0·0355 inch. Then the strings should be—

$$\begin{aligned} \text{1st} &= 0{\cdot}0237 \\ \text{2nd} &= 0{\cdot}0355 \\ \text{3rd} &= 0{\cdot}0532 \end{aligned}$$

A gut string 0·0532 inch in diameter weighs, when of the same length as a 4th string, 0·98 grm., then the 4th = 2·20 grms.

Ruffini sells sets of strings in sealed boxes, and these were found to be in about the same relative proportion to each other as the sizes indicated on the gauges sold by several makers.

The measures of a set of Ruffini's strings were found to be—

$$\begin{aligned} \text{1st} &= 0{\cdot}0265 \text{ inch.} \\ \text{2nd} &= 0{\cdot}0355 \quad ,, \\ \text{3rd} &= 0{\cdot}0460 \quad ,, \\ \text{4th} &= 1{\cdot}4100 \text{ grm.} \end{aligned}$$

* In the "Early History of the Violin Family," Engel, speaking of the Crwth, says:—"Furthermore, the contrivance of placing one foot of the bridge through the sound-hole, in order to cause the pressure of the strings to be resisted by the back of the instrument, instead of by the belly, is not so extraordinary and peculiar to the Crwth as most writers on Welsh music maintain. It may be seen on certain Oriental instruments of the fiddle kind which are not provided with a sound-post. For instance, the bridge is thus placed on the three-stringed fiddle of the modern Greek, which is only a variety of the ordinary rabáb, but which the Greeks call lyra. Inappropriate as the latter designation may appear, it is suggestive, inasmuch as it points to the ancient lyra as the progenitor of the fiddle."—P. 28.

It will be seen that the 1st string is thicker, and the 3rd thinner, and the 4th much lighter than the theoretical values. Therefore the tension of the 1st string would be greater, and that of the 3rd and 4th strings less than they should be in relation to that of the 2nd string. The greater flexural rigidity of the 4th string will have a small effect in the direction of making the vibrations quicker, and therefore of making the tension required less.

By means of a mechanical contrivance I found the weights necessary to deflect the strings to the same amount when the violin was in tune. The results agreed with the tensions which the sizes of the strings showed they would require to give fifths.

A violin strung with strings of the theoretical size was very unsatisfactory in tone.

The explanation of this departure of the sizes of the strings which long experience has shown to be practically most suitable, from the values they should have from theory, lies probably in the circumstance that the height of the bridge is different for the different strings. It is obvious, where the bridge is high, there is a greater downward pressure. By this modification of the sizes of the strings there is not the greater pressure on the 4th string side of the bridge, which would otherwise be the case. On the contrary, the pressure is less, which may assist the setting of the belly into vibration. There is also the circumstance that the strings which go over a high part of the bridge stand farther from the finger-board, and have therefore to be pressed through a greater distance, which would require more force than is required for the other strings, if the tension were not less.

II. "Note on the Atomic Weight of Glucinum or Beryllium." By J. Emerson Reynolds, M.D., F.R.S. Received May 8, 1883.

In the course of a paper by Professor Humpidge on the above subject, recently read before the Society,* the author seeks to decide between the atomic weight 9·2 for beryllium, resulting from my comparison of the atomic heat of the element with that of silver and aluminium,† and the value 13·8, arrived at by MM. Nilson and Pettersson by determination of specific heat.‡ The difference between the two possible atomic weights is so small, and the difficulties met with in

* Read April 12, 1883.

† "Chemical News," vol. xxxv, p. 124, and vol. xlii, p. 273. A slight modification of the method of comparison adopted is described in detail in the writer's "Experimental Chemistry" (Longmans), Part 1, p. 59.

‡ "Proc. Roy. Soc.," vol. 31, p. 37.

attempting to prepare even a few decigrams of beryllium are so great, that both sets of experiments have been objected to on the ground, amongst others, that the metal employed was in all cases impure. My specimen admittedly contained a minute quantity of platinum, and the proportion of known impurity in one of MM. Nilson and Pettersson's specimens reached 13 per cent. Unfortunately, Professor Humpidge's metal, though claimed to be the purest yet prepared, is shown by analysis to be rather less pure than one of the specimens employed by Nilson and Pettersson, hence the experiments lately made known to the Society do not carry the inquiry beyond the point previously reached, save in one noteworthy particular, namely, that there appears to be a considerable, though irregular, rise in specific heat of the element as the proportion of impurity diminishes; but the value is still much below that required for the atomic weight 9·2. Thus for a specimen of beryllium which contained 13 per cent. of *known* impurity Nilson and Pettersson obtained the specific heat 0·4084 between 0° and 100° C., and for a less impure specimen 0·425; while Professor Humpidge, in one of his experiments with a material that contained 6 per cent. of impurity, found the specific heat to be nearly 0·45 (0·4497). In all these cases corrections were applied which were believed to eliminate the effects due to the impurities known to be present—in part mechanically mixed with the metal and partly alloyed with it.

These results all tend in one direction, that is to say, to apparent gain in specific heat with increased purity of material, and in so far they approach the still higher value obtained in my old experiments. But even if the latter had not been made, the apparent rise in specific heat shown by the other determinations, would suggest the necessity for appeal to data afforded by beryllium of undoubted purity. In order that further experiments should now be considered decisive, the metal should not only be pure, but in the form of a homogeneous mass obtained by fusion, as the specimen I used was an apparently uncrystalline product of fusion, while the metal employed by Nilson and Pettersson chiefly consisted of "aggregations of little prismatic needles," mixed with the oxide.

The most promising source of pure beryllium is the double fluoride of the element and potassium, but I have not hitherto succeeded in making the product of reduction form a button of metal.

Professor Hartley has very recently made known some highly interesting spectroscopic evidence* affecting the position of beryllium amongst the metals, and so directly bearing on the question of its valence that I may be permitted to refer to the results in this place.

If the atomic weight of beryllium be 13·8, the element is a triad and the formula of its oxide must be Be_2O_3. The latter therefore resembles

* In a communication read before the Chemical Society, April 19, 1883.

alumina in being a sesquioxide, but is at once distinguished as it does not afford an alum-like double sulphate as do alumina and its homologues, and has comparatively little in common with that group, save the tendency to form highly basic salts. Nilson and Pettersson,* admitting this, maintain that beryllium is a leading member of another group of triads, which includes the rare earth-metals scandium, yttrium, lanthanum, didymium, terbium, erbium, &c. The recent spectroscopic evidence above referred to is opposed to this contention, as the spectrum of beryllium is stated to be wholly unlike the spectra afforded by the rare earth-metals with which it is classed in the memoir above cited. If, then, beryllium does not find a place in the two known families of metallic triads, or pseudo-triads, it must stand alone; and in any case as a triad it is outside Mendeleef's classification. But if the atomic weight of beryllium be 9·2, according to my result, the metal is a diad and the symbol for its oxide is BeO. It is, therefore, the first member of Mendeleef's second series of elements. This position is quite in accordance with the spectroscopic evidence obtained by Professor Hartley, from which he concludes that "beryllium is the first member of a diad series of elements, of which in all probability calcium, strontium, and barium are homologues."

III. "The Effects of Temperature on the Electromotive Force and Resistance of Batteries. II." By WILLIAM HENRY PREECE, F.R.S. Received May 21, 1883.

In the discussion on my previous paper read on February 22, 1883, it was suggested that I should continue the observations on the influence of temperature to the case of secondary batteries. I am indebted to Mr. Tribe for one of his cells made so as to fit my apparatus, and charged at different times with solutions of various degrees of saturation.

The negative element of this cell consisted of pure peroxide of lead in the form of a plate 4 inches square carried in a grooved frame, from one end of which projected the necessary conductor. This element was placed between two plates of finely divided lead likewise 4 inches square. These were joined together, and formed the positive element of the cell. Each half of the positive plate was about a quarter of an inch distant from the negative, and all three plates were encased in a thin specially prepared fabric. The elements were contained in a leaden case, and the liquid was sulphuric acid of the strengths given in the various experiments. This cell was placed inside the cylindrical copper vessel used in the previous experiments, and precisely the same method of observation was adopted. The results are given in

* "Proc. Roy. Soc.," vol. 31, p. 50.

DIAGRAM IV. 10 °/° SULPHURIC ACID.

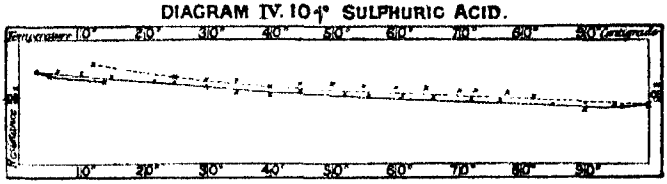


DIAGRAM V. 20 °/° SULPHURIC ACID

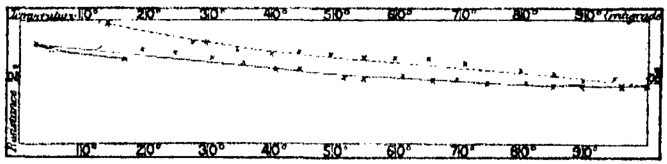
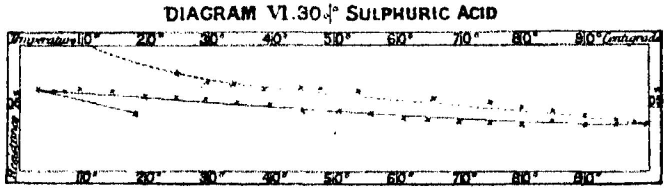


DIAGRAM VI. 30 °/° SULPHURIC ACID



Tables VII, VIII, and IX, and plotted out in the curves IV, V, and VI. The different results are for different degrees of saturation. It is evident from an inspection of those tables and diagrams, that the influence of heat on secondary cells is the same in kind as in the Daniell cell, but that it differs very much in degree. The electromotive force practically remains constant for all degrees of temperature, but the internal resistance diminishes as the temperature increases at a very steady rate, increasing again as the temperature is lowered. The effect of varying the percentage of acid in solution is not very marked, though as might have been anticipated from Kohlrausch's observations, the 30 per cent. proportion gives the lowest resistance. The mean average reduction in resistance between 0° and 100° C., is 59·6 per cent. This is shown by the following table:—

Percentage of acid.	Resistance in ohms.		Percentage of fall.
	0°.	100°.	
10	·0752	·0460	61
20	·0800	·0457	57
30	·0620	·0358	58
Mean.....	·0724	·0425	59·6

Table VII.

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 10 per cent. of Sulphuric Acid.
April 10th, 1883.

Time.		Temperature of the cell.	D.	*d.*	*d'.*	*r.*	*e.*	*b.*	Remarks.
Date.	Hour.								
April 10.	11.30 A.M.	14° C.	262	466	189	·1	1·903	·0682	
	11.50 A.M.								Cooling commenced.
		5	,,	467	197	,,	1·907	·0729	
		3	,,	467	200	,,	1·907	·0752	
	12.25 P.M.								Heating commenced.
		6	,,	463	198	,,	1·890	·0747	
		10	,,	465	196	,,	1·898	·0728	
		15	,,	464	191	,,	1·895	·0699	
		22	,,	465	188	,,	1·898	·0678	
		25	,,	465	185	,,	1·898	·0660	
		30	,,	464	180	,,	1·895	·0633	
		35	,,	463	174	,,	1·890	·0602	
		40	,,	464	172	,,	1·895	·0589	
		45	,,	464	172	,,	1·895	·0589	
		52	,,	464	168	,,	1·895	·0567	
		56	,,	465	165	,,	1·898	·0550	
		61	,,	466	163	,,	1·903	·0537	
		66	,,	468	162	,,	1·911	·0529	
		72	,,	466	157	,,	1·903	·0508	
		77	,,	467	158	,,	1·907	·0511	
		85	,,	466	152	,,	1·903	·0484	
		90	,,	466	146	,,	1·903	·0456	

Table VII (continued).

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 10 per cent. of Sulphuric Acid. April 10th, 1883.

Time.		Temperature of the cell.	D.	*d*.	*d'*.	*r*.	*e*.	*b*.	Remarks.
Date.	Hour.								
April 10.	..	96° C.	262	466	147	·1	1·903	·0460	
		100	"	469	155	"	1·915	·0493	
	1.9 P.M.								Heating stopped.
		95	"	464	150	"	1·895	·0477	
		90	"	463	152	"	1·890	·0488	
		82	"	462	161	"	1·886	·0534	
		78	"	461	165	"	1·882	·0557	
		73	"	463	167	"	1·890	·0564	
		70	"	462	169	"	1·886	·0576	
		65	"	461	172	"	1·882	·0595	
		60	"	460	172	"	1·878	·0597	
		55	"	459	172	"	1·874	·0599	
		50	"	460	172	"	1·878	·0597	
		45	"	458	173	"	1·870	·0607	
		40	"	460	176	"	1·878	·0619	
		35	"	461	182	"	1·882	·0652	
		30	"	461	185	"	1·882	·0670	
	5.0 P.M.	25	"	461	188	"	1·882	·0688	Left undisturbed till the next day.
April 11.	10.30 A.M.	12·5	"	458	203	"	1·870	·0796	

Table VIII.

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 20 per cent. of Sulphuric Acid. April 13th, 1883.

Time.		Temperature of the cell.	D.	*d.*	*d′.*	*r.*	*e.*	*b.*	Remarks.
Date.	Hour.								
April 13.	11.35 A.M.	17° C.	262	475	195	·1	1·939	·0696	
	12.0 noon.								Cooling commenced.
		5	,,	475	206	,,	1·939	·0765	
		3	,,	475	211	,,	1·939	·0799	
	*12.40 P.M.								Heating commenced.
		13	,,	476	206	,,	1·943	·0763	
		20	,,	476	200	,,	1·943	·0724	
		25	,,	475	198	,,	1·939	·0714	
		31	,,	475	192	,,	1·939	·0678	
		36	,,	476	187	,,	1·943	·0647	
		41	,,	479	180	,,	1·956	·0602	
		45	,,	479	179	,,	1·956	·0596	
		52	,,	479	171	,,	1·956	·0555	
		55	,,	478	168	,,	1·952	·0542	
		61	,,	479	168	,,	1·956	·0544	
		66	,,	479	163	,,	1·956	·0516	
		70	,,	478	160	,,	1·952	·0503	
		75	,,	478	158	,,	1·952	·0494	
		81	,,	479	158	,,	1·956	·0492	
		85	,,	478	150	,,	1·952	·0497	

Table VIII (continued).

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 20 per cent. of Sulphuric Acid.
April 13th, 1883.

Time.		Temperature of the cell.	D.	*d*.	*d'*.	*r*.	*e*.	*b*.	Remarks.
Date.	Hour.								
April 13.	..	90° C.	262	478	150	·1	1·952	·0457	
		96	„	478	150	„	1·952	·0457	
		100	„	478	154	„	1·952	·0475	
	1.25 P.M.								Heating stopped.
		95	„	478	161	„	1·952	·0508	
		90	„	479	163	„	1·956	·0516	
		85	„	475	170	„	1·939	·0558	
		80	„	476	176	„	1·943	·0586	
		71	„	475	184	„	1·939	·0632	
		65	„	474	189	„	1·935	·0663	
		60	„	475	192	„	1·939	·0678	
		55	„	474	192	„	1·935	·0681	
		50	„	474	196	„	1·935	·0705	
		45	„	473	200	„	1·931	·0733	
		40	„	474	200	„	1·935	·0730	
		35	„	475	205	„	1·939	·0759	
		30	„	472	211	„	1·927	·0809	
		28	„	472	211	„	1·927	·0809	Left undisturbed till the next day.
April 14.	10.30 A.M.	14	258	470	231	„	1·949	·0966	

Table IX.

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 30 per cent. of Sulphuric Acid. May 4th, 1883.

Time.		Temperature of the cell.	D.	*d*.	*d'*.	*r*.	*e*.	*b*.	Remarks.
Date.	Hour.								
May 4.	11.0 A.M.	18·5° C.	252	488	157	·1	2·072	·0474	
	11.15 A.M.								Cooling commenced.
	11.50 A.M.	6	,,	487	189	,,	2·067	·0634	
		3	,,	489	192	,,	2·076	·0646	
	12.10 P.M.								Heating commenced.
		7	,,	490	188	,,	2·080	·0622	
		10	,,	490	190	,,	2·080	·0633	
		15	,,	490	186	,,	2·080	·0612	
		20	,,	487	180	,,	2·067	·0586	
		25	,,	489	177	,,	2·076	·0567	
		30	,,	488	175	,,	2·072	·0559	
		35	,,	486	170	,,	2·063	·0538	
		40	,,	486	165	,,	2·063	·0514	
		45	,,	490	160	,,	2·080	·0485	
		51	,,	490	157	,,	2·080	·0471	
		56	,,	487	152	,,	2·067	·0454	
		61	,,	490	148	,,	2·080	·0433	
		65	,,	490	145	,,	2·080	·0420	
		70	,,	492	140	,,	2·089	·0398	
		75	,,	489	137	,,	2·076	·0389	
		80	,,	490	137	,	2·080	·0388	

Table IX (continued).

Effects of Temperature on Mr. Tribe's Secondary Cell, charged with 30 per cent. of Sulphuric Acid, May 4th, 1883.

Time.		Temperature of the cell.	D.	*d.*	*d'.*	*r.*	*e.*	*b.*	Remarks.
Date.	Hour.								
May 4.	..	85° C.	252	491	136	·1	2·084	·0383	
		90	"	492	132	"	2·089	·0367	
		95	"	492	135	"	2·089	·0378	
		98	"	492	135	"	2·089	·0378	
		100	"	493	130	"	2·093	·0358	
	12.45 P.M.								Heating stopped.
		95	"	490	133	"	2·080	·0387	
		90	"	490	142	"	2·080	·0408	
		85	"	488	152	"	2·072	·0452	
		80	"	488	160	"	2·072	·0488	
		75	"	488	166	"	2·072	·0516	
		66	"	488	173	"	2·072	·0549	
		54	"	487	185	"	2·067	·0613	
		48	"	483	189	"	2·050	·0643	
		45	"	483	190	"	2·050	·0649	
		39	"	477	189	"	2·025	·0656	
		34	"	474	195	"	2·012	·0609	
		30	"	480	202	"	2·038	·0727	
		25	"	477	208	"	2·025	·0773	Left undisturbed till the next day.
May 5.	10.30 A.M.	12	260	490	242	"	2·016	·0976	

IV. "Examination of the Meteorite which fell on the 16th February, 1883, at Alfianello, in the District of Verolannova, in the Province of Brescia, Italy." By WALTER FLIGHT, D.Sc., F.G.S. Communicated by Professor G. G. Stokes, Sec. R.S. Received May 17, 1883.

I gather from a short preliminary notice, which has been sent by M. Denza to Professor Daubrée, and has been published in a recent number of the "Comptes Rendus," a few particulars respecting the fall of this stone, and its general appearance.

The fall took place, with a loud detonation, at 2.55 P.M. on the day above mentioned; it was heard in the neighbouring provinces of Cremona, Verona, Mantua, Piacenza, and Parma. In Alfianello it is described as "épouvantable."

It descended from N.N.E. to S.S.W., at a distance of about 150 metres from a peasant, who fell fainting to the ground; telegraphic wires were set in motion, and the windows were shaken. It struck the ground about 300 metres south-west of Alfianello, in a field on an estate called Frosera, penetrating the soil, in the same direction as it passed through the air, from east to west, to a depth of about 1 metre, the path through the soil being about 1·50 metre. When taken out of the ground it was still a little warm. It fell complete, but was at once broken to pieces by the farmer of the estate.

The stone is oval in form, and somewhat flattened in the centre, the lower part being larger and convex, like a kettle, the upper part being truncated. The surface is covered with the usual black crust, and strewn with little cavities, now met with as individuals, now in groups, and in the eye of some people bearing a resemblance to the impression of a hand or the foot of a she-goat. The stone weighs about 200 kilos.

In structure this meteorite belongs to the group Sporadosideres oligosideres, and resembles *Aumalite*, being almost identical with the meteorite of New Concord, Ohio.

The substance is finely granular, of ash-grey colour; a polished surface appears to be finely grained and breccia-form, with the elements offering different gradations of colour. Metallic grains are disseminated, and little nests are noticed, of iron with one of the compounds, of a yellowish-white or bronze. In one place where the metallic grains are numerous they appear to bear to the stony portion the ratio 68 : 1000. The density of the stone is 3·47 to 3·50.

The meteorite was dried at 120°, and treated with solution of mercury chloride, and thus there were dissolved the troilite and nickel-iron. The troilite constituted 6·919 per cent. of the meteorite, and the nickel-iron forms 2·108 of the stone, with the composition—

Nickel	71 ·205
Iron	28 ·795
	100 ·000

Here, again, as I have shown in earlier analyses, the percentage of nickel present in nickel-iron increases as the percentage of nickel-iron becomes less.

By long treatment with hydrogen chloride the silicates acted upon by that reagent and the silicates which resist the action were separated, and the stone appeared to possess the composition—

Troilite.................	6·919
Nickel-iron	2 ·108
Soluble silicate..........	50 ·857
Insoluble silicate	40 ·116
	100 ·000

The soluble silicate, which amounts to 50·857 per cent., and constitutes one-half the weight of the stone, consists of—

Silicic acid..........	35 ·12	....	18 ·73	
Iron protoxide.......	51 ·43	....	11 ·43	16 ·37
Alumina............	1 ·518	....	0 ·707	
Lime...............	4 ·644	....	1 ·327	
Magnesia...........	7 ·269	....	2 ·904	
	99·98			

This olivine, which gives a green colour to a fragment of the rock that is at once recognised, is of unusual composition, containing as it does more than 50 per cent. of iron oxide. It agrees most closely with that which occurs in the meteorite of Ensisheim, the first recorded fall which has been preserved in any collection; it fell 17th November, 1492. The latest analysis of that stone is by Frank Crook, of Baltimore, made in Gottingen in 1868, and he found in the soluble portion of that stone 52·90 per cent. of iron oxide.

The insoluble portion, which forms 40·116 per cent. of the stone, has the composition—

Silicic acid.........	56 ·121	....	29 ·93	
Iron protoxide.......	13 ·397	....	2 ·97	11 ·95
Chromium oxide	8 ·281	....	..	
Lime...............	6 ·712	....	1 ·917	
Magnesia..........	17 ·263	....	7 ·065	
	102 ·174			

The bronzite, or rather augite, also agrees very well with that

which forms the insoluble portion of the meteorite of Ensisheim. What was supposed to be alumina was further examined, and was found to be almost entirely chromium oxide, doubtless present in combination with some iron protoxide, alumina, and magnesia as chromite. And it appears not improbable that this part of the meteorite contains some tridymite, a few per cent., in fact.

V. "Circular concerning Astronomical Photography." From E. C. PICKERING, Director of Harvard College Observatory, Cambridge, Mass., U.S.A.

ASTRONOMICAL PHOTOGRAPHY.

The important part that photography is likely to play in the future of astronomy renders it desirable that an opportunity should be afforded to astronomers to acquaint themselves with the improvements continually made in this branch of their science. This could best be done by the establishment at convenient places of collections designed to exhibit the progress of photography as applied to astronomical observations.

The Harvard College Observatory has some special advantages for forming such a collection, since it already possesses many of the early and historically important specimens which would naturally form part of the series. Among these may be mentioned four series of daguerreotypes and photographs of various celestial objects taken at this Observatory. These series were respectively undertaken in 1850, 1857, 1869, and 1882.

At present, the astronomers of the United States have no ready means of comparing their photographic work with that done in Europe, or even with that of their own countrymen. The proposed collection of photographs, so far as it could be rendered complete, would greatly reduce the difficulty.

It is therefore desired to form, at the Harvard College Observatory, a collection of all photographs of the heavenly bodies and of their spectra which can be obtained for the purpose; and it is hoped that both European and American astronomers will contribute specimens to this collection. Original negatives would be particularly valuable. It may happen that some such negatives, having slight imperfections which would limit their value for purposes of engraving, could be spared for a collection, and would be as important (considered as astronomical observations) as others photographically more perfect. In some cases, astronomers may be willing to deposit negatives taken for a special purpose, and no longer required for study, in a collection where they would retain a permanent value as

parts of an historical series. Where photography is regularly employed in a continuous series of observations, it is obvious that specimen negatives only can be spared for a collection. But in such cases it is hoped that some duplicates may be available, and that occasional negatives may hereafter be taken for the purpose of being added to the collection, to exhibit recent improvements or striking phenomena.

When negatives cannot be furnished, glass positives, taken if possible by direct printing, would be very useful. If these also are not procurable, photographic prints or engravings would be desirable.

In connexion with the photographs themselves, copies of memoirs or communications relating to the specimens sent, or to the general subject of astronomical photography, would form an interesting supplement to the collection. A part of the contemplated scheme will involve the preparation of a complete bibliography of the subject, including a list of unpublished photographs not hitherto mentioned in works to which reference may be made.

The expense which may be incurred by contributors to the collection in the preparation and transmission of specimens will be gladly repaid by the Harvard College Observatory when desired.

EDWARD C. PICKERING,
Director of the Harvard College Observatory.

Cambridge, Mass.,
February 21, 1883.

May 31, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

THE BAKERIAN LECTURE—"On Radiant Matter Spectroscopy. A New Method of Spectrum Analysis," was delivered by WILLIAM CROOKES, F.R.S. Received May 24, 1883.

The following is an Abstract:—

For several years I have been examining the phenomena presented by various substances when struck by the molecular discharge from the negative pole in a highly exhausted tube. I have ventured to call this discharge "radiant matter," and under its influence a large number of substances emit phosphorescent light, some faintly and others with great intensity. On examining the emitted light in the spectroscope most bodies give a faint continuous spectrum, with a more or less decided concentration in one part of the spectrum, the superficial colour of the phosphorescing substance being governed by this preponderating emission in one or other part of the spectrum. Sometimes, but more rarely, the spectrum of the phosphorescent light is discontinuous, and it is to bodies manifesting this phenomenon that my attention has been specially directed.

For a long time past I have been haunted by a bright citron-coloured band or line appearing in these phosphorescent spectra, sometimes as a sharp line, at others as a broader nebulous band, but having always a characteristic appearance and occurring uniformly in the same spot. The best way to bring out the band is to treat the substance under examination with strong sulphuric acid, drive off excess of acid by heat, and finally to raise the temperature to redness. The anhydrous sulphate thus left frequently shows the citron band in the radiant matter tube, when before this treatment the original substance shows nothing. I soon came to the conclusion that the substance I was in search of was an earth, but on attempting to determine its chemical properties I was baffled.

Much chemical evidence tended to support the view that the band might be due to a compound of lime. By neglecting the portion showing least citron band, and separating all the elements present which gave little or none, I could generally concentrate the citron band into a solution which—according to our present knowledge of analytical chemistry—should contain little else than the earths,

alkaline earths, and alkalies. Ammonia added to this solution would precipitate an earth, and in the filtrate oxalic acid would precipitate an insoluble oxalate, showing the citron band strongly. This was found on analysis to consist of strontic and calcic oxalates. The strontia being separated, the remaining lime formed an oxalate which gave the citron band.

So far all the chemical evidence went to show that the band-forming substance was calcium, and further tests tried with the purified oxalate confirmed this inference. Every analytical test to which it was subjected showed lime, and nothing but lime; all the salts which were prepared from it resembled those of lime, both physically and chemically; the flame spectrum gave the calcium lines with extraordinary purity and brilliancy; and, finally, the atomic weight, taken with great care, came out almost the same as that for calcium, 39·9 as against Ca 40.

On further examination it was found that most native compounds of lime gave the citron band. It was found in clear and colourless Iceland spar, native calcic phosphate, a crystal of arragonite, a stalactite of calcic carbonate from Gibraltar, cinnamon stone (lime alumina garnet), iron slag from a blast-furnace, pink coral, commercial plaster of Paris, and most specimens of ordinary burnt lime.

Evidence stronger than this in favour of the view that the citron band was an inherent characteristic of calcium could scarcely be; but, on the other hand, there was evidence equally conclusive that the band was not essential to calcium.

Starting with a lime compound which showed the citron band, I could always obtain a calcic oxalate which gave the band stronger than the original substance; but if I started with a lime compound which originally gave no citron band, I could never by any means, chemical or physical, constrain the lime or the earthy precipitate to yield a citron band.

The only explanation that I could see for this anomaly was that the elusive citron band was caused by some element precipitated with the calcic oxalate, but present in a quantity too small to be detected by ordinary chemical means. The calcic oxalate was ignited and dissolved in hydrochloric acid, and fractionally precipitated in three portions with ammonic oxalate, the first and third portions being comparatively small. They were then tested in the radiant matter tube. All three portions showed the citron band, but the portion which came down first gave the band decidedly the strongest, and the third portion precipitated showed it weakest.

It having been found that the substance giving the citron band formed a sulphate more soluble in water than calcic sulphate, 4 lbs. weight of commercial plaster of Paris, which showed very faint traces of the citron band, were mixed with water and poured on

a large filter. A few ounces of water were poured on, and after passing through, poured back, and the exhaustion repeated several times. The aqueous extract was then evaporated to dryness, ignited with sulphuric acid, ground in a mortar with small successive quantities of water, and precipitated with ammonic oxalate. The precipitate, ignited with sulphuric acid, showed the citron band very fairly, far more intensely than it was seen in the original calcic sulphate.

These experiments are conclusive in proving that the citron band is not due to calcium, but to some other element, probably one of the earthy metals, occurring in very minute quantities but widely distributed along with calcium, and I at once commenced experiments to find a more abundant supply of the body sought for. Amongst other substances tested I may note the following as giving a more or less decided citron band in the spectrum when treated with sulphuric acid in the manner indicated above:—Crystallised barytic chlorate, heavy spar, common limestone, strontic nitrate, native strontic carbonate, crystallised uranic nitrate, commercial magnesic sulphate, commercial potassic sulphate, tobacco ash, wagnerite (magnesic phosphate and fluoride), zircon, cerite, and commercial ceric oxalate.

Some specimens of zircon appeared sufficiently rich to make it probable that here might be found an available source of the citron band yielding body. I found it in crystals from Green River, North Carolina, from Ceylon, from Expailly, from Miask (Oural), and from Brevig, and having a good supply of North Carolina zircons, these were worked up by a process given in detail in the paper.

I may condense a year's work on zircon,—over 10 lbs. weight of crystals from North Carolina having been worked up—by stating that the result was comprised in about 300 grs. of an earthy residue, and about 2 ozs. of oxalate, chiefly calcic; the former gave the citron band very well.

The zirconia prepared from these zircons, when tested, sometimes showed the citron band, and at other times none. A zirconia rich in citron band, fractionally precipitated by ammonia, yielded precipitates of increasing richness, the last fraction showing the citron band strongly.

The calcic oxalate obtained from zircon gave unsatisfactory results, so attention was directed to the earthy residue. This was found to be of highly complex character, containing thoria, ceria, lanthana, didymia, yttria, and probably some of the newly-discovered rarer earths.

The position of the citron band in the spectrum falls exactly on the strongest absorption band of didymium, so that a piece of didymium glass or cell of solution of the nitrate entirely obliterates the citron band. This naturally suggested that the band was due to didymium.

Cerite was accordingly the next mineral experimented on. The powdered mineral tested in the tube in the original way gave a good citron band. The mixed earths after extraction were converted into sulphates, dissolved in water, and the cerium metals precipitated by long digestion with excess of potassic sulphate.

The precipitated double sulphates were converted into oxalates, and after ignition and treatment with sulphuric acid, the mixed ceria, lanthana, and didymia were tested in the radiant matter tube, but the merest trace only of citron band was visible.

This experiment proved the inadequacy of the didymium explanation, and further tests showed that not only could I get no citron band in pure didymium compounds, but the spectrum entirely failed to detect didymium in many solutions of the earth which gave the citron band brilliantly.

Attention was now turned to the solution filtered from the insoluble double sulphates. Potash was added, and the precipitate filtered off, and tested in a radiant matter tube. The spectrum, of extraordinary brilliancy, was far brighter than any I had hitherto obtained. Unfortunately, however, the quantity was too small to be subjected to very accurate chemical analysis.

Search was now made amongst other minerals rich in the rarer earths. Thorite was finely powdered, treated with sulphuric acid, and tested in the radiant matter tube. It gave the citron spectrum most brilliantly—equal in fact to the mixture of earths obtained from zircons at so great an expenditure of time and trouble. Orangite treated in the same manner gave almost as good a spectrum. Pure thorinic sulphate prepared by myself was found not to give the citron band, but three specimens prepared and given to me by friends all gave it, so it was not unlikely that in thorite and orangite might at last be found a good source of the long-sought element—that in fact the body I was hunting for, if not thorina, might possibly be Bahr's hypothetical wasium. Two pounds of orangite and thorite were extracted with hydrochloric acid. The solution was precipitated with potassic sulphate, taking the usual precautions to secure complete precipitation. A bulky precipitate ensued, which contained the thorina and cerium earths. These were separated and tested, and found to give only a faint citron band.

The solution of earthy sulphates soluble in potassic sulphate was precipitated with ammonic oxalate. The precipitate ignited with sulphuric acid, and tested in a radiant matter tube, gave the citron spectrum with great brilliancy.

Certain chemical facts concerning the behaviour of the sought-for element which came out during the course of the tentative trials described in the paper considerably narrowed the list amongst which it might probably be found. All the evidence tends to show that it

belongs to the group of earthy metals, consisting of aluminium, beryllium, thorium, zirconium, cerium, lanthanum, didymium, and the yttrium family, together with titanium, tantalum, and niobium. The sought-for earth is insoluble in excess of potash; this excludes aluminium and beryllium. It is not precipitated by continued boiling with sodic thiosulphate; this excludes aluminium, thorium, and zirconium. Fused with acid potassic sulphate, the resulting compound is readily soluble in cold water; this excludes tantalum and niobium. Evaporating to dryness with hydrochloric acid and heating for some time does not render the mass insoluble in water; this excludes titanium and silicium. It is easily soluble in an excess of a saturated solution of potassic sulphate; this excludes thorium, the cerium group, some of the numerous members of the yttrium group, and zirconium. The only remaining elements among which this elusive body would probably be found are those members of the yttrium family which are not precipitated by potassic sulphate.

The yttria earths form a somewhat numerous family. Fortunately for chemists, a mineral rich in yttria earths—samarskite—has been found lately in large quantity in Mitchell County, North Carolina, and to this mineral I accordingly now directed my attention.

The following list of elements of the yttrium and its allied families said to occur in samarskite and similar minerals may be considered complete to the present time.

Name.	Absorption spectrum.	Hydrogen equivalent of metal.* (Type of oxide M_2O.)
Cerium	No	47·1
Decipium	Yes	57·0
Didymium	Yes	48·5
Didymium β	Yes	47·0
Erbium	Yes	55·3
Holmium	Yes	54·0
Lanthanum	No	46·0
Mosandrum	No	51·2
Samarium	Yes	50·0
Scandium	No	14·7
Terbium	No	49·5
Thulium	Yes	56·5
Ytterbium	No	57·9
Yttrium	No	29·7
Yttrium α	No	52·2
Yttrium β	Yes	49·7

* As it is at present doubtful whether the oxides of several of the metals in this table belong to the type M_2O, M_2O_3, or MO, I have, for the sake of uniformity and

Some of these claimants it is certain will not stand the test of further scrutiny. Thus samarium and yttrium β are in all probability identical; and I have not included philippium, as Roscoe has conclusively proved that this is a mixture of terbium and yttrium, and my own results confirm those of Roscoe. Moreover, some of these so-called elements will probably turn out to be mixtures of other known elements. But in the confessedly very imperfect state of our knowledge of the chemistry of these metals it is not safe for me in this research to assume that any one of them will surely not survive. The complete list as it stands will therefore be taken to contain all hitherto claimed as new, although it is almost certain to include too many.

In the second column "Yes" or "No" indicates whether the solutions give an absorption spectrum when examined by transmitted light. After numerous experiments I satisfied myself that the metal giving the citron band spectrum was not one of those giving an absorption spectrum. The possible elements, therefore, became narrowed to the following list:—Cerium, lanthanum, mosandrum, scandium, terbium, thorium, ytterbium, yttrium, yttrium α, and zirconium.

Of these the potassic sulphate reaction excludes cerium, lanthanum, scandium, thorium, yttrium α, and zirconium, so there are left only the following:—

Mosandrum,
Terbium,
Ytterbium,
Yttrium.

Certain chemical reactions for a long time made me dismiss yttrium from the list of likely bodies. In my analysis of zircons, towards the latter part of the process, I used the following process to separate the iron:—The solution mixed with tartaric acid and excess of ammonia was allowed to stand for some time. A small quantity of a precipitate gradually formed, which was filtered off, and it was this filtrate, after separating the iron with ammonic sulphide, that yielded the greatest quantity of substance giving the citron band. Now one of the methods of separating yttria from alumina, beryllia, thoria, and zirconia is to precipitate it as tartrate in the presence of excess of ammonia, the other earths remaining in solution. Fresenius says:—"The precipitation ensues only after some time, but it is complete."

The precipitate thus obtained with tartaric acid and ammonia should therefore contain all the yttria: *it gave no citron band whatever in the radiant matter tube;* whilst the residue, which should be free from

simplicity in calculating the values from the composition of their salts, by which these metals are chiefly discriminated, taken the type of oxide to be M_2O.

yttria, proved for a long time the only source of material wherewith to investigate the chemical properties of the body giving the citron spectrum.

Another reason which made me at this stage of the research pass over yttria was that I had already tested this earth in the radiant matter tube. In a paper on "Discontinuous Phosphorescent Spectra in High Vacua," read before the Royal Society, May 19, 1881, I said, "Yttria shows a dull greenish light, giving a continuous spectrum."

For these reasons I for a long time omitted yttria from my list of possible bodies, and considered that the earth, if not a new one, might turn out to be either mosandra, terbia, or ytterbia.

About 15 lbs. weight of samarskite was worked up, partly by the hydrofluoric acid method of Lawrence Smith, and partly by fusion with potassic bisulphate.

These methods both gave as a result a large quantity of mixed earths containing most, if not all, the bodies enumerated in the foregoing list. Tested in the radiant matter tube this mixture gave the citron spectrum very brilliantly.

These earths were treated by a series of chemical processes too long and complicated to describe in this abstract, and the result of about five hundred fractional precipitations gave me a mixture of earths having an H equivalent, $M=48$, and showing a strong absorption spectrum; a mixture having an H equivalent, $M=33$, having no absorption spectrum; and intermediate earths.

In the radiant matter tube all these fractions gave the citron band spectrum well, but that of the earth of lowest equivalent was much the brightest, and that of the highest equivalent the least intense.

Three methods are available for the partial separation of these earths and for the complete purification of any one of them. The formic acid process is best for separating terbia, as terbic formate is difficultly soluble in water, the other formates being easily soluble.

Fractional precipitation with oxalic acid separates first erbia, holmia, and thulia, then terbia, and lastly yttria. This is the only method which is applicable for the separation of small quantities of terbia from yttria.

Fusing the nitrates separates ytterbia, erbia, holmia, and thulia from yttria. It is not so applicable when terbia is present, and is most useful in purifying the gadolinite earths. This process is the only one known for separating ytterbia from yttria.

Selection must be made of these methods according to the mixture of earths under treatment, changing the method as one earth or the other becomes concentrated on one side or thrown out on the other. Each operation must be repeated many times before even approximate purity is attained. The operations are more analogous to the separation of members of homologous series of hydrocarbons by fractional

distillation than to the separations in mineral chemistry as ordinarily adopted in the laboratory.

Pure terbia ignited with sulphuric acid and tested in the radiant matter tube shows no citron band spectrum. A concentrated solution of the purest terbia obtained in this way, when examined by the spectroscope, showed no absorption lines whatever: proving the absence of erbium, holmium, and thulium.

I did not attempt any separation of erbium, holmium, and thulium from each other, as the evidence obtained was sufficient to show that the element giving the citron band spectrum was not one of these three metals. Likewise I had far too little material to enable me to enter on a work of such difficulty with any prospect of success.

The chemical characters of mosandra are so little known that I could not attempt to search for it. But as the citron band-forming earth always appeared concentrated amongst those whose double sulphates were most soluble in potassic sulphate—and, of these, amongst those having the palest colour and lowest atomic weight—it was scarcely conceivable that the earth I was in search of should ultimately prove to be one whose properties did not in any case correspond to these—of a dark orange-yellow colour, forming a difficultly soluble double potassic sulphate, and having the very high equivalent of $M=51{\cdot}2$; these being the properties ascribed to mosandra by the discoverer, Professor Lawrence Smith.

Ytterbia was prepared from gadolinite, as this mineral is said by Nilson to contain most ytterbia. It was separated from accompanying earths by processes described in the paper. The resulting earth gave at first a faint citron band spectrum, evidently due to impurity; on repeating the purification several times I at last succeeded in obtaining a white earth which gave only the merest trace of citron band spectrum. Its hydrogen equivalent, 58·0, and its chemical properties showed that it was probably Marignac's ytterbia. Subsequent experiments satisfied me that this earth did not contain more than $\frac{1}{10000}$ part of yttria. The extreme tediousness of the chemical operations necessary to obtain this high degree of purity, and the long time they required, prevented me from pushing these results beyond what was necessary to prove the special point at issue.

The yttria, purified as already described, might still contain traces of terbia, together with erbia, holmia, and thulia. These were gradually removed by the fusing nitrate process. The atomic weight gradually got down to 81·0, but the spectra did not vary very much; that from the earth of lowest atomic weight being, however, the most brilliant.

Pure yttria is quite white. That from gadolinite on testing in the radiant matter tube gave a spectrum absolutely identical with that given by the zircon, cerite, thorite, orangite, and samarskite yttria.

Pure yttria was also prepared from yttro-tantalite, euxenite, tyrite, and also from plaster of Paris and common limestone. In no case could I detect any difference in the position or intensity of the lines shown by their phosphorescent spectra.

The Phosphorescent Spectrum of Yttria.

The spectrum shown by pure ignited yttric sulphate in a radiant matter tube is one of the most beautiful objects in the whole range of spectroscopy. The spectrum is best seen under low dispersion and not too narrow a slit. It consists essentially of a broad red band, an intensely brilliant citron band, and two almost equally brilliant green bands. Other fainter lines are also seen, but they are not characteristic. Coloured drawings and maps of the spectrum to scale accompany the paper. This description applies to the spectrum shown either by pure yttric sulphate or by an earth tolerably rich in yttria. When traces are present the citron band only is seen. A little more yttria brings out the first and then the second green band, and finally, as the proportion of yttria increases, the red and blue bands appear.

The paper next gives a description of experiments made with pure yttria, and with various compounds of it, to see which would give the most characteristic spectrum. The sulphate heated to redness was found to give the best results. Pure yttria precipitated by ammonia did not phosphoresce in the slightest degree, and, necessarily, no citron band spectrum was to be seen. The yttria was removed from the tube, converted into sulphate, heated to redness, and again tested. This time it gave the citron band magnificently. This shows what apparently trivial circumstances will alter the whole course of an investigation. In 1881, when searching for discontinuous phosphorescent spectra, I tried a similar experiment with pure precipitated yttria, and entirely missed its citron band spectrum. Had I first treated the yttria with sulphuric acid instead of testing the earth itself in the radiant matter tube the results would have been very different, and this research would probably have never been undertaken.

Yttria was now prepared by igniting the precipitated oxalate at a red heat. On testing it in the radiant matter tube it phosphoresced with feeble intensity, the light being about one-twentieth of that given by the ignited sulphate under similar conditions. The citron band was almost as sharp as the sodium line, and was shifted one division towards the blue end. The two green bands were visible, but very hazy and indistinct, and only to be resolved into bands with difficulty.

It is an old and probably a true saying that every element could be detected everywhere had we sufficiently delicate tests for it. Early observations had prepared me for the wide distribution of the element

giving the citron spectrum, and no sooner had the exquisite sensitiveness of this spectrum test forced itself on my notice than I sought for yttrium in other minerals. The facts which I had noticed in connexion with the variation of the appearance of the citron spectrum, according to the quantity of yttrium present, showed that it might be possible to devise a process for the rough quantitative estimation of yttrium, and after several experiments a spectrum test was devised sufficiently delicate to detect one-millionth part of yttria in a mineral. A table is given showing the results of this quantitative spectrum analysis, from which it is seen that amongst other substances a specimen of coral contains one part of yttrium in 200 parts; strontianite, one part of yttrium in 500 parts; chondrodite, from Mount Somma, one part in 4,000; calcite, one part in 10,000; ox bone, one part in 10,000; an earthy meteorite (Alfianello), one part in 100,000; and tobacco ash, one part in 1,000,000.

The following Paper was read:—

"Experiments upon the Heart of the Dog with reference to the Maximum Volume of Blood sent out by the Left Ventricle in a Single Beat, and the Influence of Variations in Venous Pressure, Arterial Pressure, and Pulse Rate upon the Work done by the Heart." By WILLIAM H. HOWELL, A.B., Fellow of the Johns Hopkins University, and F. DONALDSON, Jr., A.B. Communicated by Dr. M. FOSTER, Sec. R.S.

(Abstract.)

Owing to the indirectness of the methods hitherto used for estimating the quantity of blood pumped out from the left ventricle at each systole, this important factor in all calculations of the work done by the heart has never been satisfactorily determined. Of the later physiologists who have investigated the subject, Volkmann and afterwards Vierordt, from calculations based upon the mean velocity of the stream of blood in the unbranched aorta, obtained the fraction $\frac{1}{400}$ as representing the ratio of the average weight of blood ejected at each systole of the left ventricle to the weight of the whole body. Fick, from data obtained by placing the arm in a plethysmograph, and estimating the velocity of the stream of blood in the axillary artery from the increase in volume of the whole arm at each systole of the heart, arrived at a much smaller fraction, about $\frac{1}{1000}$, for the ratio between the weight of blood thrown out at each systole and the body-weight.

At the suggestion of Professor Martin, and under his directions, we undertook some experiments upon this subject, making use of his method of isolating the heart. The quantity of blood ejected from the left ventricle at each systole under varying conditions of venous pressure, arterial pressure, and pulse-rate, can be determined directly with this method by catching the blood as it is pumped out from the tube connected with the aorta of the dog. In all the observations made the blood was collected during a period of 30 seconds, and the quantity thus obtained divided by the number of heart beats occurring during that time, as shown on the kymograph, in order to determine the quantity pumped out at each systole. The results of our work fall under four different heads.

I. *The Maximum Quantity of Blood which can be thrown out from the Left Ventricle at a single Systole.*

The method of working in determining this quantity was to increase the amount of blood flowing into the right side of the heart, by raising the supply flask connected with the superior vena cava, until a limit was reached in the amount of blood pumped out from the left ventricle, *i.e.*, a point beyond which increase of the pressure and the quantity of the blood flowing into the right side of the heart caused no increase in the quantity of the blood sent out from the left ventricle.

The main result of these experiments may be stated at follows: With a mean pulse-rate of 180 per minute in the dog, the mean ratio of the maximum weight of blood pumped out from the left ventricle at each systole to the body-weight is $\frac{1}{855}$, or ·00117. The maximum outflow from the left heart was obtained in all cases at or below a venous pressure on the right side of 60 centims. of defibrinated calf's blood (46 millims. of mercury).

With regard to the maximum outflow from the left ventricle at the normal pulse-rate (120 per minute) of a dog, we have only one experiment to offer. According to that experiment the ratio of the maximum weight of blood pumped out from the left ventricle at each systole to the body-weight, with a pulse-rate of 120 per minute, is about $\frac{1}{700}$, or ·0014.

In applying these results to the normal dog, we believe that the average quantity of blood pumped out from the left ventricle at each systole in the living dog is approximated most nearly in our experiments by the maximum outflow; in other words, we think it probable that the left ventricle during life is distended at each diastole to about its maximum capacity. In coming to this conclusion we were influenced chiefly by three considerations. 1. The same opinion has been expressed by Royar, the result of his work on the frog's heart. 2. Calculations of the time required for a complete

circulation of the blood to take place in a normal dog, based upon the maximum quantity of blood sent out from the left heart at each systole, as determined by our experiments, give results which agree with what we would expect from the time found necessary by Vierordt from direct experiment for the jugular-femoral path. 3. It is probable that in our experiments the left heart, at the time the maximum outflow was obtained from it, worked under conditions of pressure closely resembling those to which it is subject during life. The pressure in the left auricle, with the highest venous pressure (46 millims. of mercury) used on the right side, had a maximum value of 20 millims. of mercury, a mean value of 16 millims., which is probably about what occurs during life, since Goltz and Gaule found that the maximum pressure in the right auricle of the dog is about 19·6 millims. of mercury.

Owing to the difference in pulse-rate between the dog and man, no inference can be made with any certainty from the results obtained with the dog to the case of man.

II. *Influence of Variation of Arterial Pressure on the Work done by the Heart.*

Arterial pressure was varied by raising or lowering the end of the outflow tube leading from the aorta. As the result of the experiments we can state that variation of arterial pressure from 58 to 147 millims. of mercury, have practically no direct effect whatever upon the quantity of blood sent out from the left ventricle at each systole.

Since the pulse-rate is not altered, the work done by the left ventricle, therefore, varies directly as the arterial pressure against which it works, within the limits named. For how much wider limits than those given this may hold true we have not yet determined. Since there is every reason to believe that under normal conditions the force of the systole is more than sufficient to completely empty the ventricular cavity, and we have found that with arterial pressures from 58 to 147 millims. the quantity of blood pumped out from the left ventricle at each systole remains constant, it seems probable that within these limits at least the force of the ventricular contraction is not influenced by variation in arterial pressure, but remains maximal throughout.

III. *Influence of Variation of Venous Pressure on the Work done by the Heart.*

The venous pressure on the right side was gradually increased from 10 centims. to 60 or 70 centims., and the outflow from the aorta measured for each venous pressure used. The records of these experiments show, in a marked manner the influence of venous pressure on the outflow from the ventricle. As the general results of

the experiments, it was found that the outflow from the left ventricle, and consequently the work done by it, increases with the venous pressure, but not proportionally, up to the point of maximum work. It is certain that the most direct factor influencing the quantity of blood sent out from the ventricle is the intra-ventricular pressure by which it is distended during diastole. Leaving out the aspirating action of the thorax, the intra-ventricular pressure during life must be mainly owing to the action of the auricles, since the pressure in the great vein emptying into the auricles probably never rises to any important positive value, indeed, according to the experiments of most observers, has a mean negative value. The contraction of the auricles then must have the most important and direct effect upon the work done by the ventricles.

IV. *Influence of Rate of Beat on the Work done by the Heart.*

The rate of beat of the heart was varied in these experiments by heating or cooling the blood supplied to it. In this way the pulse-rate was changed in one case from 228 to 77 beats in a minute, and back again to 140 in a minute, in another case from 204 to 65·5, and back again to 157, and so on. The general result may be stated as follows:—A diminution of pulse-rate, brought about by lowering the temperature of the blood flowing into the heart, causes an increase in the quantity of blood thrown out from the ventricle at each systole, and consequently an increase in the work done at each systole, and *vice versâ.* The changes in the outflow from the ventricle at each systole are not, however, inversely proportional to the changes in the pulse-rate. The total outflow and the total work done during any given period of time decreases with a diminished pulse-rate, and increases with an increased pulse-rate.

June 7, 1883.

The Annual Meeting for the Election of Fellows was held this day.

THE TREASURER, V.P., in the Chair.

The Statutes relating to the election of Fellows having been read, Sir James Cockle and Sir Henry Lefroy were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society.

Aitchison, James Edward Tierney, Surgeon-Major, M.D.
Browne, James Crichton, M.D., LL.D.
Dobson, George Edward, Surgeon-Major, M.A., M.B.
Duncan, James Matthews, A.M., M.D.
Fitzgerald, Prof. George Francis, M.A.
Flight, Walter, D.Sc.
Frost, the Rev. Percival, M.A.
Gill, David, LL.D.
Groves, Charles Edward, F.C.S.
Grubb, Howard, F.R.A.S.
Langley, John Newport, M.A.
Reinold, Arnold William, M.A.
Trimen, Roland, F.L.S., F.Z.S.
Venn, John, M.A.
Walker, John James, M.A.

Thanks were given to the Scrutators.

June 14, 1883.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Dr. James Crichton Browne, Surgeon-Major George Edward Dobson, Dr. James Matthews Duncan, Mr. Charles Edward Groves, Professor Arnold William Reinold, and Mr. John James Walker were admitted into the Society.

The following Papers were read:—

I. "Researches on the Foraminifera. Supplemental Memoir. On an Abyssal Type of the Genus *Orbitolites*; a Study in the Theory of Descent." By W. B. CARPENTER, C.B., M.D., F.R.S. Received May 31, 1883.

(Abstract.)

This paper is supplemental to the series of Memoirs formerly presented by the author on the structure of certain of the higher forms of the group of Foraminifera; in which he laid down the new principles of classification afterwards worked out by him, in conjunction with Messrs. W. K. Parker and T. Rupert Jones, in his "Introduction to the Study of the Foraminifera;" and especially to the first of those memoirs ("Phil. Trans.," 1856), which consisted of a Monograph of the genus *Orbitolites*, and of some general doctrines deduced from the study of it as to the Range of Variation in Species, a question which was much occupying the attention of philosophical naturalists. The subsequent publication of Mr. Darwin's "Origin of Species" having led him unhesitatingly to adopt the principle of "Descent with Modification" or "Genetic Continuity," he had applied it to the construction of a pedigree of the *Orbitoline* type; between the smallest and simplest, and the largest and most complex of which, he had shown in his first memoir that such "continuity" could be clearly traced out.

Starting with the very simplest type of Foraminiferal organisation—a minute globular or pyriform monothalamous shell, with a single orifice, enclosing a particle of sarcode—he showed (1) that the extension of such a particle into a spirally-coiled sarcodic cord, invested by a porcellanous shell, becomes a *Cornuspira*; (2) that a constriction of this cord at the points at which additions are made to the length of

the shell, with a partial interruption of the tube at these points, gives us *Spiroloculina*, the ancestral form of the whole *Milioline* series; (3) that the flattening-out of this tube (as in the more advanced growth of *Cornuspira*), and the formation of a complete septum across its mouth, traversed by separate pores for the issue of sarcodic filaments, convert it into a *Peneroplis*, whose sarcodic body consists of a succession of segments, occupying the successive chambers of the shell, which are divided by septal partitions, but are connected by "stolon processes" that traverse them; (4) that the subdivision of these chambers by transverse partitions into "chamberlets," and of the segments of the body into "sub-segments," the shell still growing along a spiral axis, gives us *Orbiculina;* the chamberlets of each chamber retaining a communication with each other by a continuous gallery, which is occupied in the living state by a band of sarcode that connects together all the sub-segments formed by the division of any one segment; and (5) that the opening-out of the Orbiculine spire, and the progressive extension of the *alæ* of its successively-formed chambers round its umbilicus, at last brings these *alæ* together, so that they unite into a continuous ring; and by the addition of new rings to the periphery of the preceding, a circular disk is formed—the plan of growth thus changing from the spiral to the *cyclical*, which is the distinguishing character of *Orbitolites*.

The materials at that time possessed by the author only enabled him to trace back this pedigree with any certainty from the typical *Orbitolites*, which exhibits no trace whatever of spiral growth, to the spiral *Orbiculina;* but he expressed his belief that the "nuclear mass" in which every *Orbitolites* originates—consisting of a pyriform "primordial segment," surrounded by a single turn of the "circumambient segment," is essentially an abbreviated *Milioline;* and he was thus led to rank *Orbitolites* as the most specialized type of the family MILIOLIDA.

It was, therefore, with singular satisfaction that he found in a new form of *Orbitoline* disk, brought up from a depth of 1,500 fathoms in the "Porcupine" expedition of 1869, the complete realisation of his hypothetical pedigree: the formation of this disk commencing in a minute primordial chamber, which first extends itself into a closely coiled spiral tube like that of a *Cornuspira*, then shows an incipient septation in the later coils of this tube, which constitutes it a *Spiroloculina;* then flattens-out and becomes camerated as a *Peneroplis;* then undergoes the subdivision of its chambers which converts it into an *Orbiculina;* and, finally, by the fusion of the lateral extensions of the chambers into complete annuli, assumes the *cyclical* plan of growth characteristic of *Orbitolites*. To this beautiful species, whose disk attains a diameter of $\frac{6}{10}$ of an inch, whilst its thickness does not exceed $\frac{1}{800}$ of an inch, the name *Orbitolites tenuissima* may be appropriately given.

The passage of each individual of this species through a series of forms which, in the classification of M. D'Orbigny, belong to *four different Orders*, sufficiently proves that, among Foraminifera, *plan of growth* is a character of secondary value; whilst the retention, in the perfected disk, of the entire series of those ancestral forms, through which the very simplest of Foraminiferal organisms has become evolved into one of the most complex, invests this abyssal type of *Orbitolites* with a peculiar interest and value.

Having been for some time engaged in the study of the large collection of *Orbitolites* (chiefly made on the Fiji reef) brought home by the "Challenger," with a view to the preparation (at the request of the late Sir C. Wyville Thomson) of a complete Report upon this generic type, the author has delayed the publication of this remarkable confirmation of his previously-expressed views, until he should have concluded his investigation of the large mass of new material which has thus come into his possession; having early seen reason to believe that his recent study would prove of some value in its general relation to the Theory of Descent. And he now offers the results of it as a contribution to that great inquiry, in the conviction that (as was admirably said thirty-five years ago by Sir James Paget) "the highest laws of our science are expressed in the simplest terms, in the lives of the lowest orders of creation."

The aggregate of the phenomena presented by the evolutionary history of this type (and equally, the author fully believes, by that of every other of the more complex types of Foraminifera) may be thus summed up:—

1. That there has been a progressive specialization in the structure of the shelly envelope, which, in the highest type of *Orbitolites* (the large *O. complanata* of the Calcaire Grossier and of the Fiji reefs) attains a very extraordinary complexity.

2. That this specialization has followed a very definite and well-marked line.

3. That this progressive complication in the structure of the disk is attained without any corresponding specialization in the structure of the animal, whose sarcodic body retains throughout (as far as the most careful examination can enable us to determine) its primitive homogeneousness.

4. That all the ancestral forms through which the highest type has passed, are still living and flourishing under exactly the same conditions (so far as can be ascertained) as itself.

The full discussion which the doctrine of the "Origin of Species by Natural Selection" has now received, may be considered as having clearly established that what has been called the Law of Natural Selection is simply a generalised expression of the fact, that among the varetal forms continually arising *de novo*, those survive

which are best adapted to their environments. The causes of such variation, not being in any way accounted for by "natural selection," must be looked-for either in the influence of the "environment" on the organism, or on some tendency to vary inherent in the organism itself; and the question which now most occupies the minds of thoughtful Evolutionists, is whether the variations that have conduced to the establishment of the higher types are "aimless," or whether they have followed a definite "plan."

From a careful consideration of all the circumstances of this case, the author comes to the conclusion not only that such a "plan" can be clearly traced out in the present case, but that "natural selection" can have had scarcely any share in determining the progressive evolution and relative distribution of the several forms of the *Orbitoline* type.

II. "On the Development of the Great Omentum and Transverse Mesocolon." By C. B. LOCKWOOD. Communicated by W. S. SAVORY, F.R.S. Received May 18, 1883.

(Abstract.)

The paper begins by quoting the two usually accepted descriptions of the peritoneum as far as the relations of the omentum and transverse colon are concerned. The old account, that which makes the transverse colon to be between the two ascending layers of the great omentum, is first given. Afterwards the new account is repeated; this says that the colon is not between the layers of the great omentum, but only adherent to them. The development of the colon is next mentioned, and Haller's theory, that the colon and omentum become adherent, is discussed. Reasons are given to show that the old account of the peritoneum is the true one, and that, therefore, Haller's theory is unacceptable. After speaking of the development of the omentum and its relation to the transverse colon, the changes which the author believes to occur are described. Instead of adhesion taking place between the omentum and colon, it is shown that the peritoneal fossa, which at early periods exists between them, gradually disappears, owing to an unfolding or drawing out of the peritoneum at that point. It is further shown that when this has taken place the transverse colon comes to be between the two ascending layers of the great omentum.

A brief description of the anatomical preparations which accompany the paper is also given.

III. "On the Ciliated Groove (Siphonoglyphe) in the Stomodæum of the Alcyonarians." By SYDNEY J. HICKSON, B.A., B.Sc. Assistant to the Linacre Professor, Oxford. Communicated by Professor MOSELEY, F.R.S. Received May 23, 1883.

(Abstract.)

1. In Alcyonium there is a groove lined by remarkably long cilia, situated on the ventral side of the stomodæum. This groove, which has already been superficially referred to by O. and R. Hertwig, has important morphological relations in the group Alcyonaria which have not been previously referred to. I propose to call it the siphonoglyphe.

2. The cilia of the siphonoglyphe, as seen in a living Alcyonium, moving in unison, produce a current from without inwards which brings particles of food and fresh streams of water into the canal system of the colony. The cilia lining the rest of the stomodæum produce currents in an opposite direction, from within outwards.

3. A siphonoglyphe, varying in size and in the length of the cilia, is present in the same position in all the non-dimorphic Alcyonarians (without solid calcareous or horny axes) I have examined, *e.g.*, Cœlogorgia, Briareus, Nephthya, Spongodes, Tubipora, Clavularia, Heliopora, &c.

4. Amongst the dimorphic Alcyonarians the siphonoglyphe is usually absent in the autozooids, but well developed in the siphonozooids. In Sarcophyton, however, a feebly-developed siphonoglyphe is present in the autozooids in addition to the well-developed ones in the siphonozooids.

5. In Primnoa and Villogorgia, the only examples of Alcyonarians with solid axes I have examined, no siphonoglyphe can be found, and I am inclined to think, from the researches of other observers, and from general considerations, that it is not present in any genera in which the fleshy parts of the colony are represented only by a thin crust covering solid axes.

6. The paper contains some speculations to which I have been led, by these researches, concerning the probable phylogeny of the group, and a diagrammatic arrangement of the Alcyonaria on these lines.

7. Finally I propose to divide the Alcyonaria into five principal groups: 1st. The Proto-Alcyonaria, including only those genera which do not form colonies. 2nd. The Stolonifera, including the genera Clavularia, Cornularia, Tubipora, &c., in which the young colonies spring from a creeping stolon. 3rd. The Pennatulidæ, which remains as heretofore. 4th. The Gorgonidæ, a group which contains only those genera in which there are solid horny or calcareous axes,

and no siphonoglyphe. 5th. The Alcyonidæ, a large and somewhat heterogeneous group containing all the remaining genera of the Alcyonaria, which, though exhibiting many wide variations, *inter se*, agree in possessing no specially marked characters of deviation from an ideal central form from which, I suppose, they must have sprung.

IV. "On the Variations of Latency in certain Skeletal Muscles of some different Animals." By THEODORE CASH, M.D. and GERALD F. YEO, M.D. Communicated by Dr. SANDERSON, F.R.S. Received May 29, 1883.

In a former paper ("Proc. Roy. Soc.," vol. 33, p. 462) we laid before the Society the results of a series of experiments by which we had endeavoured to ascertain accurately the differences in the duration of the latent period of contraction of skeletal muscle (frog's gastrocnemius) which could be brought about by varying the following influences:—

1. The weight of load.
2. The mode of applying weight (supported or unsupported).
3. The strength of stimulation.
4. Temperature.
5. Fatigue.

We have since been engaged in determining the relative duration of the latent periods of different skeletal muscles of vertebrate animals. Besides several muscles of the *Rana temp.*, we have examined some from the toad, tortoise, small mammals, and birds. In this paper, which is intended to be a continuation of the one above referred to, we beg leave to lay before the Society the results of these experiments and our general conclusions.

We know from the works of Fick,* Marey,† Ranvier,‡ Frédéricq,§ Richet,|| and one of us,¶ that various muscles in the same animal have a mode of contraction differing more or less from one another, and adapted to the kind of work they have to perform. But in the works of most of these authors little information can be found concerning the variations in the duration of the latent period, in differently contracting muscles, whether those of the same animal or those of different animals.

* Fick, "Irritabelen Substanzen."
† Marey, "Du Mouvements dans les Fonctions de la Vie."
‡ Ranvier, "Leçons sur le Système Musculaire."
§ Frédéricq, "Bull. de l'Acad. roy. de Belgique," lvii, No. 6.
|| Richet, "Physiologie des Muscles," &c.
¶ Cash, "Journal of Anat. and Phys.," vol. xv.

As our desire was to obtain information of the latency actually occurring in the muscle, we adopted the same plan in this series as in our former experiments of stimulating the muscle directly. The whole length of the muscle was introduced into the secondary circuit, and a contraction registered, resulting from a maximal opening shock of Du Bois Reymond's inductorium with a single pint Daniell cell. The arrangement of the apparatus was similar to that employed in our former series of experiments.

We shall tabulate the mean results of a series of measurements of curves obtained from a number of observations upon the animals above mentioned, and we believe they will prove, that while the latencies for different muscles of the same animal bear amongst themselves certain proportion to the length of their succeeding curves, we can by no means reason from one group of animals to another what this relationship may be.

Though the duration of the latency of each individual muscle varies within certain limits in different frogs, we think we can vouch for the correctness of the inter-relationship of the results furnished by the various muscles employed in arriving at the averages given. Our measurements confirm with considerable exactitude those obtained by one of us from experiments made with the muscles of *Rana esculenta*,* in which another method was employed. We have naturally regarded those results as most satisfactory, in which, by working rapidly, we succeeded in obtaining representative curves of the whole series of muscles furnished by one animal.

Frog.

Table I.

Rana Temporaria.—Examined in December. The average of the measurements obtained from a large number of experiments is given.

Muscle.	Weight.	Length of latency.	Altitude.	Length of curve.
Gastrocnemius ..	10 grms.	·0117″	23 mm.	·12″
Triceps	10 „	·0114″	30 „	·112″
Semimembranosus	10 „	·0116″	28 „	·119″
Hyoglossus......	5 „	·015″	29 „	·18″
Rectus abd......	5 „	·014″	33 „	·167″
Biceps crur......	5 „	·0124″	18·5 „	·12″

* Cash, "Journal of Anat. and Phys.," vol. xv.

Toad.

Table II.

The averages of several experiments made during December and January. Fresh specimens which as a rule had been but one night in captivity were examined. (The weather having been very mild during the time of experimentation the animals were active and vigorous.)

Muscle.	Weight.		Length of latency.
Gastrocnemius..	10 grms.	....	·0123″
Triceps	10 „	....	·0121″
Hyoglossus	6 „	....	·0191″
Rectus abd.	6 „	....	·0191″
Biceps crur.	6 „	....	·0135″
Sartorius	6 „	....	·015″

March.—From perfectly fresh *Toads* the following variations in values were obtained. The animals had not spawned :—

Muscle.	Variation of length of latency.		
Gastrocnemius	·0135″	....	·0146″
Triceps	·0128″	....	·0128″
Hyoglossus	·015″	....	·0192″
Rectus abd.	·013″	....	·015″
Biceps crur.	·015″	....	·0155″
Sartorius	·0137″	....	·0146″

The durations of the latencies in the latter part of Table II differ but slightly from those obtained from the "winter frog," as will be seen from contrasting the following muscles :—

Muscle.	(Fresh) Toad (March.)		Frog (December).
Gastrocnemius	·0135″	....	·0117″
Triceps............	·0128″	....	·0114″
Hyoglossus	·015″	....	·015″
Biceps crur.........	·015″	....	·0124″
Rectus abd.	·013″	....	·014″

It is worthy of note that the more pronounced differences of latency between toad and frog lie in the muscles of the extremities; the trunk muscles yield more nearly equal values. An appeal to function shows that the circumstance is highly probable. The frog which depends upon speed of movement for procuring its food, for safety from enemies, and preservation from death in times of drought, has different motor requirements from the toad, which depends for safety

on crouching, and on its approximation of colour to that of surrounding objects, whilst it rather waits for its food to come to it, than attempts to pursue it. But the requirements of the trunk muscles are much more similar in the two animals, and thus we find the hyoglossus for procuring food, and the rec. abdominis for spawning, respiration and flexion of the body, yielding very similar values.

Tortoise.

In order to obtain a complete record of the long curve of contraction of the tortoise muscle, and at the same time an exact estimation of its latency, a second recording surface had to be employed. The apparatus was therefore so arranged that the tendon of the muscle drew upon the membrane of a Marey's tambour, which acted as a weight beneath the lever which recorded on the plate of the pendulum. By means of a second inverted tambour armed with a light lever, the transmitted movement was written on a rotating cylinder. The commencement of the curve including the time of latency, was drawn on the rapidly moving plate of the pendulum, and simultaneously a graphic record of the shape and duration of the total curve was obtained on the drum. The results could be associated after measurement of the tuning-fork curves, by means of which the time was in each case controlled.

Tortoise.

Table III.

Muscles of Land Tortoise. Examined in Winter.

The muscles were prepared as rapidly as possible with their bony insertions or attachments to the carapace preserved. Stimulation direct maximal.

Muscle.*	Weight.	Latency.	Length of curve to notch.	Length of curve to abscissa (*circa*).
Omohyoid	30 grms.	·0225″	·83″	4″—6″
Semimembranosus	30 ,,	·0247″	1″	4″
Biceps fem.	10 ,,	·028″	1·16″	5″
Ext. dig. com. ..	10 ,,	·033″	1·3″	5·5″

Of these muscles the first two are strictly parallel fibred. The omohyoid is the agent chiefly concerned in the retraction of the head,

* For nomenclature of muscles, see Bojanus, "Anatome Testudinis Europææ," Vilnæ, 1849.

the most rapid movement of which the animal is capable. The semi-membranosus passes over three joints, and a considerable extent of contraction is necessary in order to enable it to flex the foot on the knee, and the thigh on the pelvis. Both of these muscles arrive speedily at a maximum of contraction, ·18″ and ·24″, and coincidently with this rapid action we have the relatively short latency quoted. There is a considerable difficulty in estimating the length of curve of the tortoise muscle, owing to its gradual return to the base line. Usually a notch in the descending part of the curve shows the distinction between the active curve and the after shortening which is so marked a feature in the action of the muscles of this animal; but occasionally the one passes into the other by insensible gradations. We have, however, endeavoured by varying the burden to produce some indication of this notch when none at first existed, and we have confined ourselves to the results which we believe to be correct.

Warm-blooded Animals.

The muscles of warm-blooded animals were examined *in situ*, the circulation being as little as possible interfered with, and the animal kept well under the influence of anæsthetics. A modification of Ranvier's rabbit-holder was utilized in order to support the animal in the necessary relationship to the recording lever. The head-holder at one end of a small board, consisted of a loop of metal covered with cord, which received the snout or beak and anterior part of the head, whilst a piece of string run through two openings in the loop, and behind the occiput kept the head in position, and facilitated the administration of ether. The board which served to support the animal was perforated with numerous holes, through which strings attached to the animal passed. The under surface of the board was fitted with a ball and socket joint, which could be clamped firmly to the sliding table on which the lever rested. By a screw which fixed the cup around the ball, this joint could be tightened when the board was adjusted to any given position, and thus the muscle to be examined could be brought into direct line of traction with the lever without altering the attachment of the animal or otherwise disturbing the apparatus. The axis of the lever was provided with a short arm projecting on the side opposite to the lever, to which a thread passing from the muscle was attached. The arm was drawn downwards by the contracting muscle, and the lever therefore upwards, so that a curve in every way comparable to those obtained by direct traction was drawn.

Mammals.

Table IV.—Adult Rat.

The gastrocnemius of the rat draws a curve with a double summit.

The value of the curve to the first distinct fall of the lever is ·13″, but relaxation occurs slowly after this.

Muscle.	Weight.	Latency.	Length of curve.
Gastrocnemius ..	30 grms.	·011″	·13″

The maximum of contraction is reached in about ·03″.

Table V.—Kitten (two months old).

Muscle.	Weight.		Latency.
Gastrocnemius	30 grms.	....	·018″

Birds.

The muscles of three birds were examined.

Table VI.

Animal.	Muscle.	Weight.	Latency.	Length of curve.
Hen	Gastrocnemius ..	30 grms.	·0204″	·116″
Pigeon	"	30 "	·012″	·074″
	Pect. maj.........	30 "	·011″	·083″
Blackbird ..	Gastrocnemius ..	..	·0125″	·081″
	Biceps brach......	..	·014″	·087″
Max. of biceps brach. ·045″.				

We will not recapitulate the results given in the tables further than by drawing attention to the extremes of the values obtained. In the December frog we found the triceps directly stimulated had the shortest latency (·0114″), whilst the hyoglossus had the longest ·015″. The muscles of the trunk hyoglossus and rectus abdominis (compound but parallel fibred muscle) exhibited longer latencies than did the muscles of the extremities; the muscles of the arm again longer than those of the leg.

In forming our conclusions concerning the duration of the latent period of contraction of skeletal muscles, and the variations it is subject to, we must refer repeatedly to our former paper, of which this, as has been said, may be regarded as the continuation.

The latencies of gastrocnemii of *Rana temp.*, examined between January and April, and selected at random, varied between wide limits, namely from ·008″—·0208″. These values include also measurements taken from curarised gastrocnemii, but as we have already pointed out, the small dose of curare employed appeared in no way to cause any divergence in reaction of the muscle from the normal. In this respect our results confirm the view expressed by Pflüger. In

support of this we may mention that the shortest latency referred to occurred in a curarised, the longest in a non-curarised muscle. In the case of the toad, variations due solely to the individual were considerable, but the divergencies were not so great as in the case of the frog. Thus in six gastrocnemii taken from different individuals the range was from ·0125″ to ·017″, in the triceps (six muscles) from ·012″ to ·015″, and in the hyoglossus ·015″ to ·024″. We seldom found any material difference in the latencies of corresponding muscles in the same animal. The toads examined in December and January yielded latencies averaging ·0121″ (triceps) and ·0191″ (hyoglossus and rectus abdom.), whilst the March animals gave ·0128″ (triceps) as the shortest, and ·015″ (hyoglossus and biceps) as the longest latencies. The subjects of the latter experiments were unspawned animals, and the reaction of the trunk muscles was marked by an accession of irritability.

In the tortoise, the long parallel fibred muscles, omohyoid and semibranosus, yielded the shortest latencies, viz., ·0225″ and ·0247″ respectively, whilst the wide bellied biceps and the broad but short extensor communis digitorum gave distinctly longer periods (·028″ and ·033″). The gastrocnemius of a kitten, aged two months, showed a latency of ·018″, and that of a white tame rat ·011″.

In the pigeon the pectoralis major has a slightly shorter latency (·01″) than the gastrocnemius, but in the blackbird the biceps (a muscle intimately connected, as is the pectoralis, with flying movements) appears to have a latency longer by ·0015″ than the gastrocnemius of the same bird. It is worthy of remark, however, that the blackbird had been reared and kept in confinement, and therefore it is probable that the muscles connected with flight were to a certain extent uneducated and undeveloped.

To sum up the variations in latency obtained between the frog muscle, which, free from exceptional influences, yielded the shortest (·008″), and the tortoise muscle, which yielded the longest (·033″), we have but a range through ·025″, a variation small enough to be in itself surprising, but still more so when we consider the relative durations of the contractions to which these latencies are initial. We may certainly infer from these results that, though the intralatent processes in the various muscles of a given animal have a certain inter-relationship with the resulting contractions, and that each muscle variety gives divergencies from its normal latency and contraction only within certain limits, that we can establish no argument of probabilities from an animal of one species to an animal of another species as regards the latency preceding contraction. The same holds true of warm-blooded as well as of cold-blooded, of Carnivora as of Rodentia.

The literature published during the last twenty years relating to the duration of the latent period, bears witness to the very different lines

of investigation pursued by experimenters, as well as to the widely varying results which they have obtained. It seemed to us, therefore, that time devoted to a systematic examination with one exact method of the many aspects of the subject would be advantageously employed, and would furnish results relatively correct, and of a value which would be greater than could be obtained by grouping the conclusions of numerous authors, whose methods and objects of research differed and who had no unanimous starting point for their investigations.

For the gastrocnemius of a frog:
Helmholz* gives as a latency.................... ·01″
Place† and Gad‡ .... ·004″

whilst Mendelssohn,§ who has admirably grouped the results obtained by these and other authors, found that in frogs taken at random of various sizes and in different seasons, the latency varied within the wide limits of ·004″—012″. Navalachin‖, who has ably investigated the production of heat during the active contraction of striated muscle, has pointed out that the latency of the frog's gastrocnemius is shorter in the spring than in the summer months. Here we have to do with another element rather than with temperature—namely, with the spawning of the animal, the most disturbing internal influence to which a frog is liable. The latencies before and after spawning are various, the irritability of the muscles being different. This is in its effect almost convertible with the experience of Helmholz, Engelmann, Lautenbach, and others,¶ who have pointed out that increase in strength of stimulation shortens the latency. In the spring-time the irritability of the animal is increased, and maximal stimulation, which in the summer frog would produce a certain definite effect, is here hyper-maximal (so to speak) and the latency is correspondingly shortened. To the same reason, *i.e.*, an increase in irritability, Mendelssohn looks for the explanation of the shortened latency of the muscle, whose nerve has just been divided. We must, however, confess that we are unable to confirm his results, *i.e.*, that the latency in the case of the muscle of which the nerve has just been divided, may be temporarily shortened by half, in the case of the *Rana temp.* The latency given by Gad (·004″) associated with a lengthening of the muscle as precursor to its general contraction, we cannot attempt to criticise, as we have very rarely ourselves seen the form of curve he represents as being normal, but which he only

* Helmholz, "Müller's Archiv," 1850.
† Place, "Nederland Archiv v. Genees. en Natuur," III, 1867.
‡ Gad, "Ueber das Latenzstadium, &c.," "Archiv f. Anat. und Phys.," 1879.
§ Mendelssohn, "Marey's Travaux," 1878–9.
‖ Navalachin, "Myothermische Untersuchungen," "Pflüger's Arch.," Bd. XIV.
¶ See Tables IV and V in former paper.

obtained where the muscle is transfixed by the lever. An intra-latent lengthening of the entire muscle does occasionally occur, but in our experiments it had no constant relationship whatever to the time of stimulation, and we therefore regarded it as a chance extension. We have not, however, been able to find time to repeat the experiments—using his methods—from which Gad deduces his results.

The brief latency that Place and Klunder obtained from stimulation of the already shortening muscle, we have rarely seen. When it does occur, its accurate estimation becomes extremely difficult, as it is necessary to separate the moment when active contraction commences from the passive shortening which preceded it, and one phase often passes into the other by almost insensible gradations. Fallacies of so extensive a nature arise from the use of a slowly moving recording surface, and of a short lever, that we have in all cases employed a rapid swing of the pendulum myograph, and a lever multiplying the contraction by six or seven times, for the production of the curves from which our data of latencies are taken. In the fatigued muscle, for instance, commencing contraction is very gradual; so gradual in fact that its earlier phases may, with a short lever, be readily mistaken for a prolongation of the latency. We need not point out that with a comparatively slowly moving cylinder slight inaccuracies in measurement—at all times subject to occur—are multiplied proportionally as the speed becomes slower.

A cursory examination of the tables in our first communication shows that of the causes therein examined, the most potent in affecting the latency are variations in temperature and fatigue; of secondary importance are the strength of stimulation and the mode of suspension of the extending weight.

Temperature.—In the gastrocnemius of an "April frog" (indirect stimulation) we saw for an excursion through 20° C. (viz., from 5° to 25°) a variation of ·014″ in the length of the latency; in the muscle of another animal heated from the normal room temperature (17°) through 14° C., a variation of ·01″ was obtained. Taking the room temperature as the starting point, we find in these experiments that lowering the temperature through 5° increased the latency ·004″, ·0033″, and ·0027″ respectively. Heating through 5° above the normal yielded a shortening of ·0016″—·0012″, but though these figures represent a usual result, occasionally there is a much more extensive variation. Thus, in one case with an unusually long latency (·017″) at the room temperature, heating through 5° reduced the latency ·01″. After this point in the heating process had been reached, the course of the latency under higher temperatures was strictly comparable with that of other muscles with a more ordinary initial latency, and we should regard this circumstance as furnishing a proof that this abnormally long latency was in reality owing to

some unstable cause not usually operative. We have seen such muscles allowed again to cool to the room temperature fail to give again the long initial latency at first manifested, whereas, as a rule, the result obtained from a given temperature for the same muscle is constant or nearly so, should heating and cooling not have been carried to a deleterious extent. We cannot regard a muscle, showing a long latency and a long curve, as being in both respects in series with a more quickly contracting muscle, cooled down through a certain range of temperature. One of the most important features in the divergence, is in the change of elasticity in the case of the cold muscle, but beyond this we have variations of kind between muscles which must help to account for the intimate relationship between the latency and curve in any given muscle.

Fatigue.—M. Ranvier has recorded the remarkable fact that the latency of the thoroughly fatigued red muscle of the rabbit, may bear the proportion to that of the fresh muscle of nearly five to one. He says, if the strength of the stimulation is increased, the latency shortens again, as contractibility is redeveloped. Since the fatigued muscle is equally elastic, but less irritable than the fresh muscle, a stimulation which at first produces a maximal result, soon becomes sub-maximal, then minimal, and finally ceases to be effective; so that constant increase of stimulation is required to elicit an equal (in the sense of a maximal) effect. Mendelssohn has described an increase of latency from ·008″ to ·03″ in the gastrocnemius of the frog as the result of fatigue. These extensive variations are quite possible, but it not infrequently happens that the length of the latency is rather apparent than actual as a result of fatigue. A careful examination of the curve of a fatigued muscle will often enable us to determine a very gradual but definite departure of the lever attached to the muscle from the abscissa, in what, at first sight, seemed to be the actual latency. This would be concealed by a thick abscissa, or lost by the use of a too slightly amplifying lever.

In our former paper we gave tables showing that 1,300 induction shocks (producing maximal single contractions), increased the latency ·00526″, and that two minutes' tetanus had the effect of lengthening this period ·0066″. We also pointed out that it is in the extreme of fatigue that the rapid increase in the latency occurs. Exercise short of distinct fatigue was seen even to shorten the latency to a slight extent. This may have been in part due to the increase of extensibility which Volkmann has shown occurs in earlier stages of fatigue of the muscle. In the later stages of fatigue, however, the extensibility decreases as the elasticity of the shortened muscle increases, and here we have a corresponding *increase* in the latency.

The effects of variations of strength of stimulation and in the amount of weight employed, and the manner of employing it, have

been discussed in our previous paper, and need not occupy us here; from a physiological point of consideration these are in the economy of the living animal liable to smaller variations, and therefore of less interest than the effect of change of temperature and fatigue.

Physiologically considered, the weight raised when the muscle is in service will be constant for the individual, when the stimulations are equal. The researches of Navalachin have shown that most work is done and less fatigue produced when stimulation sub-maximal in character is applied to the frog's muscle, and no doubt the muscle in service during life is usually excited by sub-maximal stimulation (in an electrical sense). It is important to bear in mind that a constant weight used in a series of experiments bears a different relationship to the muscle of a frog weighing x grms. or x + or x − grms., inasmuch as it affects variously the elasticity of the stronger or weaker muscle. In highly extensible parallel-fibred muscles with a small limit of elasticity, such as the hyoglossus, the divergencies obtained by incommensurate weightings are very considerable.

Our experiments tend, we think, to support the idea which has been expressed by many physiologists, that the relationship between length of latency and the conditions of elasticity and extensibility of the muscle is a close one.

Conclusions.

1. The limits within which the normal latency varies (intentionally introduced extrinsic influences apart) appear to be as follows:—

a. In gastrocnemii of various frogs at different seasons—

Rana temp. ·008″—·0208.″

b. In different muscles of the same animal—

Frog ..	·008″ (gastrocnemius)	to	·022″ (hyoglossus).
Toad ..	·012″ (triceps)	„	·024″ (hyoglossus).
Tortoise	·022″ (omohyoid)	„	·036″ (ext. dig. com., &c.).

(The complete exclusion of certain influences, nutritive, thermal, &c., is impossible.)

2. Conclusions as to the length of the latent period based on the duration of the contraction are liable to error; especially is this the case when we attempt to reason for one class of animals from the relationship found in another.

3. The duration of the latency increases and decreases in direct but unequal proportion to the amount of weight which the muscle has to lift.

4. Increase in strength of stimulation is accompanied by a shortening of the latent period. The variation seems to depend on the absolute strength of the stimulus employed, viz.: (*a*) Increase from minimal to stimulation giving rise to maximal contraction is accom-

panied by a steady and marked decrease in the duration of the latency. (*b*) When once maximal contractions are arrived at, considerable increase in strength of stimulation does not alter the length of the latency. (*c*) After a certain point has been reached, further increase in strength of stimulus (hyper-maximal) causes elongation of the latent period associated with signs of injury to the tissue.

5. Fatigue must attain a considerable degree before it materially affects the length of the latency. When it once begins to produce an effect it rapidly lengthens the latent period of muscles removed from the animal or in which circulation has ceased.

6. Changes in temperature, even minimal in amount, cause a marked alteration in the latency.

Lowering of temperature is accompanied by a steady elongation, and elevation by a rapid shortening of the latent period. When the heat becomes intense (for frog over *circa* 36° C.) the length of the latency seems again to increase, as the muscle passes into heat rigor.

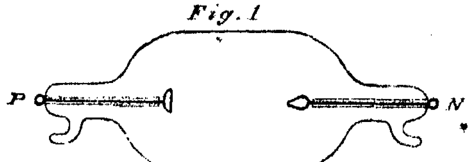
7. In observing the above variations in the duration of the latency, we have failed to find the wide extremes given by some authors as the limits of this phase of the contraction of striated muscle

V. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery. Part IV." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received June 11, 1883.

(Abstract.)

The authors recall that at the conclusion of the third part of their researches,* they stated that they intended to make an investigation on the dark discharge, and the special conditions of the negative discharge; this paper contains a number of experiments, more especially on the latter subject, and also others intended to throw light on the general nature of the electric discharge through gases.

Fig. 1



The first part of the paper describes some experiments made with vessels of different forms in order to ascertain whether the dimensions

* "Phil. Trans." vol. 171, p. 65.

and shape of the vessel have any effect on the pressure of minimum resistance to the electric discharge. This was found to be the case, for example, with a residual air charge in a spheroidal vessel 7 inches (17·8 centims.) long, and 5 inches (12·7 centims.) diameter (fig. 1), the pressure of minimum resistance was as high as 3 millims., 3947 **M**; while in a tube 22·5 inches (57 centims.) long, and 1·625 inches (4·1 centims.) diameter (fig. 2), it was only 0·69 millim., 908 **M**; again in a

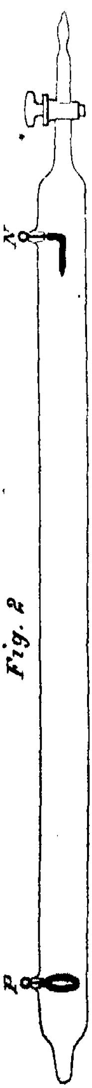


Fig. 2

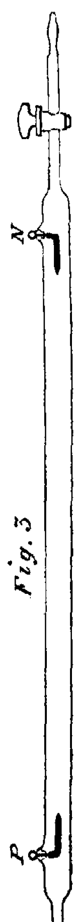
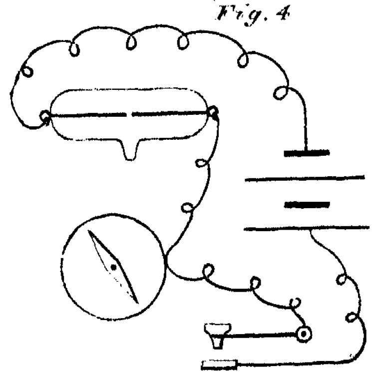


Fig. 3

smaller tube 23 inches (58·4 centims.) long, and 0·75 inch (1·9 centims.) diameter (fig. 3), it was 1 millim., 1316 M. It is evident, therefore, that not only the dimensions of the tube, but possibly also the shape of the terminals, have an influence on the pressure of least resistance, and it is very probable that in the atmosphere, where lateral expansion is practically unlimited, the conditions of minimum resistance are different from those which exist even in very large tubes, and that this may influence the height of the aurora.

The paper next deals with the discharge in miniature tubes $\frac{7}{8}$ inch (2·2 centims.) long, and $\frac{1}{4}$ inch (0·63 centim.) diameter, with terminals nearly touching (fig. 4); at first it required 2,400 cells to pass,

Fig. 4



then a single cell would do so, but after standing a short time it required 4,800 cells to reproduce a discharge. In another tube $1\frac{3}{4}$ inches (4·4 centims.) long, and $\frac{3}{8}$ inch (0·95 centim.) diameter, with the terminals distant 0·00104 inch (0·0264 millim.), it required 2,240 cells to produce a discharge, then the potential had to be increased to 11,240 cells to do so. Ultimately even this number failed, but after the tube had lain by for some days, 600 cells could pass. It is very possible that the strong discharge in the first instance volatilized a portion of the terminals which were of platinum, and that this volatilized metal condensed afterwards, or else that the terminals absorbed the gas so completely as to produce a vacuum too perfect to admit of a discharge taking place; and that, ultimately, sufficient of the occluded gas was again given off to render it again possible.

In connexion with the occlusion of gas by terminals, a case is described in which the terminals are of palladium, and the charge hydrogen (fig. 5). After a few discharges the terminals occluded some of the gas, and when a fresh one was produced, a volatile compound of hydrogen and palladium was given off, especially from the negative, and produced a dense mirror-like coating on the inside of the tube

(fig. 6); this was re-occluded by standing for a couple of days, leaving the tube free, and again given off to form a new mirror-like coating with a fresh discharge; this property has continued since March, 1875.

Fig. 5.

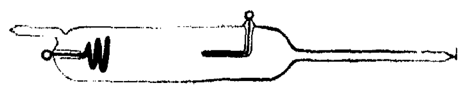
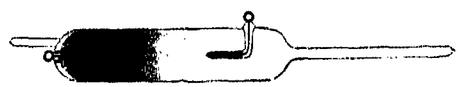


Fig. 6.



The paper next describes experiments to ascertain the length of the spark in dry air and in air saturated with moisture. It was found to be practically the same in both cases. With 10,860 cells the mean length of the spark between two paraboloidal points was found to be in dry air 0·45 inch (1·1 centims.), in moist air 0·447 inch (1·1 centims.).

The next subject taken up is the discharge in a tube from two batteries, first in the same, and then in contrary directions. In the tube are two terminals at each end, one pair at opposite ends being enclosed in two short pieces of tube, 9 inches (22·8 centims.) long, and $\frac{1}{2}$ inch (1·27 centims.) diameter; the main tube being 31 inches (95·2 centims.) long and $1\frac{3}{4}$ inches (4·4 centims.) diameter (fig. 7). The various phases of the stratified discharge are represented in an engraved mezzotint steel plate copied from photographs, and show the effect of the one stratified discharge on another stratified discharge produced by a second battery. It is seen that two discharges in contrary directions may take place in the same tube, and that the one modifies the aspect of the other.

Experiments are also described in a tube in the form of a cross with four arms at right angles (fig. 8); with two separate batteries connected in various ways with the different members. The experiments were made both in air and in hydrogen. By the introduction of external resistance in one of the batteries, the discharge could be readily identified as belonging to that battery by the effect of the resistance on the character of the stratification. In one of the mezzotint plates are several figures copied from photographs which show clearly the phenomena produced. Calling the poles P and N, of one battery, A, and P′ and N′ of the other, B, it is shown in one case when two currents were equal 0·0083 ampère, that a discharge from A battery goes from P in the direction of N only so far as the junction at the cross and then turns off to N′, the negative of the

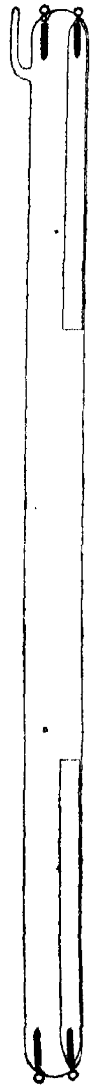
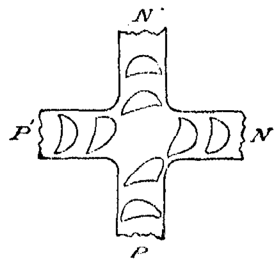


FIG. 7.

other battery B; while, on the other hand, the discharge of the B battery goes from P′ to N of the A battery. The case is different if an external resistance is introduced in one of the discharges, reducing it to 0·00087 ampère, then the discharge of the A battery goes from P to N, and that of the B battery from P′ to N′. There is a bending down, however, of the strata of the weaker discharge at the cross junction, in consequence of the action of the stronger one.

FIG. 8.



The authors remark that one cannot but be impressed, from the experiments described in the paper, and others in their former papers, by the apparent plasticity of the aggregate assemblage of molecules constituting a stratum which yields to external influences that modify its form.

The authors describe and figure a case of complex strata in the form of an outer bracket convex towards the negative (fig. 9), and close to it

FIG. 9.

an inner chord; also discharges in various gases in tubes of large dimensions, 37 inches (94 centims.) long and $5\frac{13}{16}$ inches (14·8 centims.) diameter. In these the stratification, which is comparatively narrow at the terminals, extends in a conical form from the terminals to the full diameter of the tube.

They have found that the dark space in the discharge in vacuum tubes is only relatively actinically dark in comparison with a stratum, and they succeeded in obtaining a photograph of the dark space in thirty-five minutes as strong as that from a stratum in two and a-half seconds; consequently they conclude that the dark space is 840 times less actinically bright than a stratum.

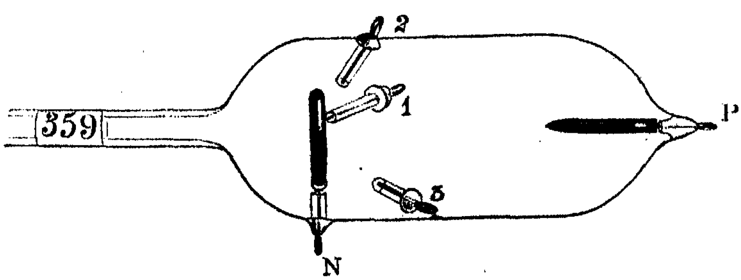
The authors next describe a number of experiments, by means of a Thomson-Becker electrometer used on a method, to avoid leakage, proposed to them by Professor Stokes, to ascertain the difference of potential in different parts of a vacuum tube having a number of rings sealed within it, also in other tubes of special construction. These bring out instructive information in reference, not only to the relative resistances of different lengths of a column of gas at various pressures, but also forcibly to the impediment presented by the

terminals themselves to the passage of a discharge from gas to terminal and terminal to gas.

It is shown that, at moderate exhausts, the resistance to the passage of the discharge is uniform along the length of the column of gas, and that at high exhausts it is not so, and that the total resistance increases but slightly with an additional length of the column; moreover, that, at these low pressures, the main impediment is in the passage of electricity between gas and terminal or terminal and gas; this is much greater at the negative than at the positive terminal.

The authors have next studied the electrical condition of a gas in the immediate vicinity of the negative terminal. In order to do this they constructed a tube $4\frac{1}{2}$ inches (11·4 centims.) long and $1\frac{7}{8}$ inches (4·8 centims.) diameter. One terminal is in the form of a point, the other in the form of a ring. The positive pole of the battery was connected with the point and the negative either to the ring alone or to earth as well; the ring terminal of the tube was, when the battery was insulated, connected with earth either by means of a stout wire or 3 feet (91·4 centims.) of fine platinum wire, 0·002 inch (0·005 centim.) diameter, and offering a resistance of 81 ohms, or a moistened cork offering a resistance of 4,300,000 ohms. In the tube were sealed three idle wires, 1, 2, 3, covered with the exception of their extremities with fine glass tubing (fig. 10). No. 1 idle wire is 0·002 inch

Fig. 10.

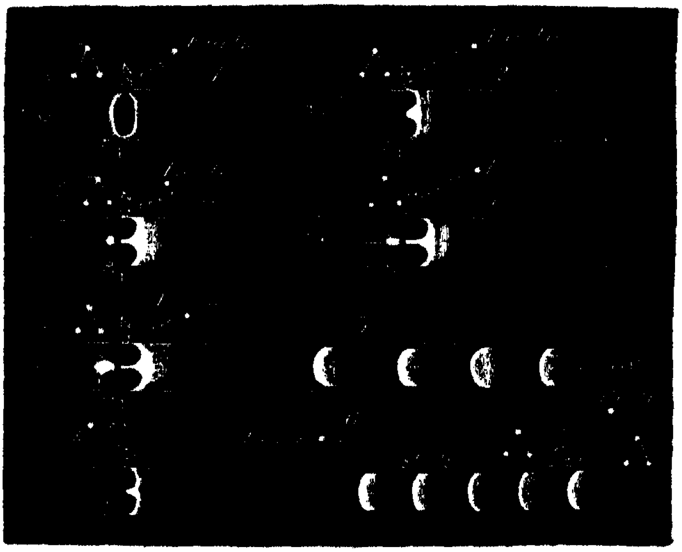


(0·005 centim.); No. 2 0·2 inch (0·5 centim.); and No. 3 0·6 inch (1·5 centims.) from the ring. The ring terminal, when connected to earth, was found to be always at zero potential; notwithstanding this there was frequently observed, more especially as the exhaust was increased, a negative potential when the idle wires were connected successively with the electrometer, amounting in one case with an air charge, pressure 0·01 millim., at wire No. 2, to 1,068 cells, at wires 1 and 3 to 912 cells. At other times a plus potential was observed. Many experiments were made to determine the precise conditions which developed a negative potential or a positive potential, but unsuccessfully, and it was inferred that this depended on the

condition of the discharge itself within the tube. It is certainly very remarkable that while the potential of the negative ring was absolutely zero, a high negative potential should be developed in its near vicinity.

The authors remark that everyone familiar with the appearance of a stratified discharge will have noticed when the negative terminal is a ring, that as the exhaust proceeds a spindle of light approaches, and at last protrudes through the interior of it (fig. 11, 1, 2, 3, 4, 5); this spindle they regard as a visible exponent of strong action among the molecules of gas composing it. In order to probe its electrical condition, they prepared a tube with a central idle wire, surrounded by a minute glass tube, except its extremity, and projecting to a distance of $\frac{3}{8}$ inch (0·95 centim.) from the plane of the ring, which was made negative. Another idle wire was sealed in the tube 0·15 inch (0·38 centim.), from the periphery of the ring. As the exhaust proceeded

FIG. 11.



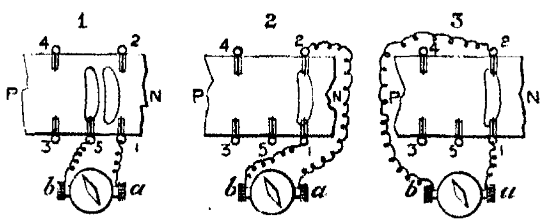
with a charge of carbonic anhydride, the spindle approached the ring, and ultimately protruded through it. It was found that the potential of the central idle wire increased with the exhaust, until it nearly or quite equalled that of the whole tube; while that of the external idle wire was only 0·054 that of the tube.

A great number of experiments were made to test the potential across a stratum *a*, *b*, and across a dark space *c*, *d*, respectively, by

two idle wires sealed in suitable positions in a tube, one of which was connected with earth, the other with the electrometer (fig. 11, 6). The gases used were carbonic anhydride and hydrogen respectively. As a mean of a great number of experiments it was found that when a dark space was straddled, the potential being reckoned 1, then when a stratum was straddled, the potential was 1·243, 1·229.

On testing two idle wires distant $\frac{5}{8}$ inch (1·6 centims.) apart with a Thomson galvanometer, the current in this fractional part of a tube was found to go frequently in the reverse direction to that of the main current, and when the galvanometer was connected to two idle wires diametrically opposite, currents were indicated sometimes in one direction, sometimes in another across the tube (fig. 12). These experi-

FIG. 12.



ments seem to indicate that there are eddies in the gas during a discharge, as if the motion of the molecules conveying an electric discharge was of an epicycloidal character. The authors conclude by saying that it is possible that the eddies may be connected with the production of strata.

June 21, 1883.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor George Francis Fitzgerald, Dr. Walter Flight, Mr. John Newport Langley, and Mr. John Venn were admitted into the Society.

The following Papers were read:—

I. "On Line Spectra of Boron and Silicon." By W. N. HARTLEY, F.R.S.E., &c., Royal College of Science, Dublin. Communicated by Professor STOKES, Sec. R.S. Received May 28, 1883.

In the course of an extended examination of all varieties of saline solutions by means of the spark and a photographic camera, I have observed two spectra of much interest. I detach my notes from the paper in which they are embodied in order to give them an earlier publication.

Boron.—In order to ascertain whether sodium borate would yield any spectrum beyond that due to sodium, a strong solution of borax was first examined and subsequently a saturated solution of boracic acid. The graphite electrode with which the solution was submitted to the action of the spark, was opposed to a pole of a tin-cadmium alloy, in order that the wave-lengths of any lines that might appear could be determined by reference to those of tin and cadmium. It is a remarkable fact that when a saturated solution of borax is used, the sodium lines are not visible, while there appear three strong sharp lines, which as they are likewise yielded by boracic acid must be considered as characteristic of boron.

The Spectrum of Boron.

Scale numbers.		Wave-lengths.
96·18	....	3450·3
269·20	....	2497·0
269·48	....	2496·2

Silicon.—A strong solution of sodium silicate was in like manner submitted to the action of the spark. There was only a feeble indication of the strongest sodium line ($\lambda = 3301$), but a strong

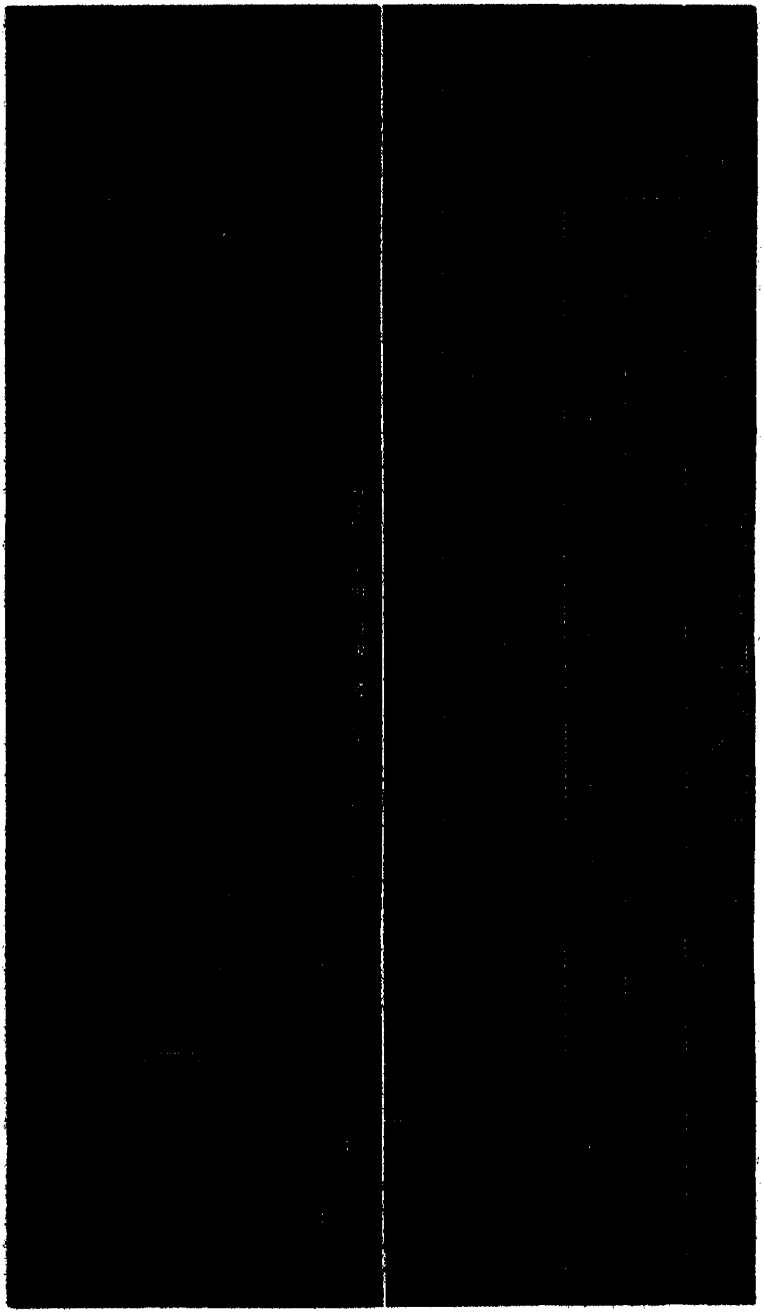
spectrum was obtained consisting of a beautiful group of lines with three isolated rays. These lines are attributed to silicon, because they are rendered equally well by sodium silicate, sodium fluosilicate, and by hydrofluosilicic acid, the electrodes being either of gold or of carbon. The strength of the lines is proportional to the strength of the hydrofluosilicic acid solution examined. The fiducial lines of the tin-cadmium alloy and some of the air lines were employed as before in obtaining measurements from which the wave-lengths of the silicon lines were calculated by means of an interpolation curve. Below are given the wave-lengths of the silicon lines, together with their scale numbers referring to their position on the prism spectrum. The scale numbers are comparable with those given in a paper recently submitted to the Royal Society, and are also applicable to my photographs of spectra in the "Journal of the Chemical Society," vol. 41, p. 90. They represent hundredths of an inch and fractions thereof, reckoned from a strong air line with wave-length 4628·9, which is numbered 10. ("Measurements of the Wave-lengths of Lines of High Refrangibility in the Spectra of Elementary Substances": Hartley and Adeney.)

The Spectrum of Silicon.

Scale numbers.		Wave-lengths.
178 ·98	....	2881 ·0
233·17	....	2631 ·4
256 ·78	....	2541 ·0
260 ·36	....	2528 ·1
261 ·65	....	2523 ·5
263 ·07	....	2518 ·5
263 ·98	....	2515 ·5
264 ·44	....	2513 ·7
266 ·54	....	2506 ·3
288 ·00	....	2435 ·5

These are the first spectra of boron and silicon obtained from metallic salts.

In Messrs. Liveing and Dewar's map of the carbon spectrum ("Proc. Roy. Soc.," vol. 33, p. 403), I have observed a group of lines not seen in the spectrum of graphite obtained by me ("Journal of the Chemical Society," vol. 41, p. 90), which might be accounted for by a difference in strength of the spark employed. This group, however, resembles in a striking manner the seven lines in the spectrum of silicon. (See the map of the silicon spectrum.) Their wave-lengths are the following—2541·0, 2528·2, 2523·6, 2518·7, 2515·8, 2514·0, 2506·3. It will be seen by comparison that these lines approximate so closely to those of silicon that the numbers are well



within the experimental errors of measurement of identical lines. Professors Liveing and Dewar took the lines which they mapped from sparks passed "between poles of purified graphite in air, carbonic acid gas, hydrogen, and coal-gas. The same lines have been observed in photographs of the spark between iron, and between aluminium poles in carbonic acid gas." "The graphite was purified by being stirred in fine powder into fused potash, and subsequent treatment with *aqua regia*, by prolonged ignition in a current of chlorine, and by treatment with hydrofluoric acid." "Notwithstanding the purification the photographs of the spark between these electrodes still showed very distinctly the lines of magnesium and iron."

From these quotations it will be seen with what great care the preparations for these observations on the carbon spectrum were made. If the poles employed had been those of graphite only, I should have had little hesitation in attributing the seven lines to the silicon spectrum, but they were replaced by iron and by aluminium. Even the purest iron wire contains small traces of silicon, and aluminium of the usual commercial quality certainly contains a considerable quantity. There is, therefore, a suspicion that the carbon spectrum was contaminated by silicon, for a series of seven consecutive lines so nearly coincident with those in the spectrum of another element of the same class would be very remarkable.

II. "On the Steady Motion of a Hollow Vortex." By W. M. Hicks, M.A., Fellow of St. John's College, Cambridge. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received May 31, 1883.

(Abstract.)

The investigation to which this refers forms a continuation of some researches commenced about three years ago, but which the author was compelled by other engagements to lay aside. The general theory of the functions employed was published in the "Transactions of the Royal Society" (Part III, 1881), under the title of "Toroidal Functions." These and analogous functions are employed in the present communication.

The interest of investigations of the properties of small vortices depends on their connexion with the vortex atom theory of Sir W. Thomson, and it was this and the further connexion with a gravitation theory which induced me originally to undertake the investigation. So far as I am aware, very little has been done towards a quantitative theory of vortices beyond the paper "On the Vibrations of a Vortex Ring" by Mr. J. J. Thomson, published in the "Transactions" of this

Society (Part II, 1882), and in which is considered the vibrations in the form of the axis of a solid core, and the action of two vortices on one another. In the present communication I have confined myself to the case of a single *hollow* vortex in an infinite fluid, and to vibrations symmetrical about the straight axis. The reason why I have chosen to begin with the hollow vortex is given below.

The vortex-atom theory, as presented by Sir W. Thomson, has always seemed to me to labour under two difficulties. It does not explain the gravitation of the atoms, nor does it afford, so far as one can see, any means of explaining the different densities of the various elements. When the exceedingly small density of the ether compared with what we call ordinary matter is considered, it is clear that the supposition that matter is composed of vortices of the same density as the ether is surrounded with great difficulties, and we are driven to the conclusion that, if a vortex-ring theory be the true one, the cores of the vortices must be formed of a denser material than the surrounding ether, and that probably this core has rotational motion. The theory of gravitation propounded by me in the "Proceedings of the Cambridge Philosophical Society"* only necessitates that the circulation or cyclic constants of the vortex-atoms shall exceed a certain amount which depends directly on the mean pressure and density of the ether. It needs, therefore, no additional hypothesis to the theory of Sir W. Thomson, but flows naturally from it. This is not the case with the explanation of difference of density here offered, as the simplicity of the theory is to some extent lost by having two elementary matters in the place of one.

With these views on the probable constitution of matter I have attempted the problem of determining mathematically the properties of a hollow vortex in an incompressible fluid, lined with an interior layer of a different density from the surrounding fluid. When the density is the same as the surrounding fluid, and the interior hollow vanishes, we have the vortex-ring of the ordinary theory as a particular case. An extreme case in the opposite direction is when there is no internal layer and no rotational motion in the fluid at all; merely the cyclic motion about a ring-shaped hollow. This is the case considered in the present communication.

If we can argue from the case of two spheres, two vortices making forced pulsations of the same period will attract one another with a mean force whose principal part depends on the inverse square of the distance, when they are in the same phase, and repel one another according to the same law when in opposite phases. When the periods are not the same they will alternately repel and attract, and the mean effect, so far as the forces depend on the inverse square, will

* "On the Problem of Two Pulsating Spheres in a Fluid," "Proc. Camb. Phil. Soc.," vols. iii and iv.

vanish. If the pulsations of two atoms have natural periods, different or not, but modified by the action of each other, then there will be a residuary effect also inversely as the square of the distance, but whether attractive or repulsive remains to be seen. It is possible that gravitation may be due to such a residuary action. The force depends on the time of pulsation and the amplitude. For a hollow vortex without core the time for the same atoms depends on the square root of the logarithm of the ratio of the radius of the section to the radius of the aperture, and would therefore vary slowly with alterations of energy. This, therefore, is an objection to the theory that gravitation depends directly on the actual pulsation rather than on the residuary effect mentioned above. The amplitude would naturally vary with the energy, and this would make the attraction between two masses alter with the temperature. On this point no experiments have been made, and the only apparent argument against it that I can think of are Kepler's laws for the motion of the planets. If this theory were true the squares of the periodic times would not vary simply as the cubes of the mean distances, but also as a quantity depending on the mean temperature of each planet. But this is no decisive argument against it, as those distances are not themselves accurately known; are, in fact, determined by this law from the earth's distance from the sun, determined by other methods.

The vortex-atom theory has its most interesting connexions with the explanation of the spectral lines of the elements. These lines, *so far as they depend on the vibrations of single atoms*, might arise from several different kinds of vibrations of the form of the ring. Thus, the hollow ring can have—

(1.) *Deformations of the circular axis:*—these must, as has been shown by Sir W. Thomson,* be such that the axis at any time is deformed into a helix wound on the surface of a tore, or the ring is *twisted*. This mode of vibration for a solid core of the same density as the surrounding fluid has been investigated by Mr. J. J. Thomson in the memoir referred to above.

(2.) *Waves running round the surface of the ring*, so that any cross-section is crimped into small elevations and depressions, and the ring itself is *fluted*.

(3.) *Vibrations of the aperture.* This is really a case of (2).

(4.) *Pulsations of the hollow*, whereby the volume of the hollow is periodically altered.

(5.) *Swellings of the ring*, travelling in one direction or the other round the ring, so that the ring seems to be *beaded*.

Cases (2) and (4), when the ring is moving steadily, are discussed in this paper.

It would be venturesome to draw conclusions in the present state

* "Vortex Statics," "Proc. Roy. Soc. Edin.," ix.

of our knowledge respecting the properties of these atoms, or attempt to find analogies even with the ordinary kinetic theory of gases. For instance, the vortex-atoms are polar, and therefore do not behave towards one another indifferently for all modes of approach. Clearly, also, the temperature of a gas composed of vortex-atoms could not depend on the translatory velocity of mean square, but would depend in some way on the mean energy. In this connexion it is interesting to notice that the time of vibration of a ring in class (2), when at least the ring is moving steadily, is independent of its energy, depends in fact only on the constants of the ring, the fluid, and the inverse square root of the number of crimps in a cross-section. If relations are to be sought between spectral lines they would arise from classes 1, 2, 5. But from Mr. Thomson's investigations it would appear that in the case of a solid tore, the time of vibration in class (1) would depend on the temperature.

Section I of the paper is devoted to a consideration of the functions employed in the investigation. In Section II is considered the motion of a rigid tore in fluid moving parallel to its straight axis. In Section III, the problem of the steady motion of a hollow vortex is taken up, together with the small vibrations of classes (2) and (4) above. It will be sufficient to give here a short abstract of the results arrived at in Section III. The cross-section of a ring is throughout considered as small compared with the aperture, and the expressions giving the form of the hollow and the velocity of translation are carried to a second approximation, the quantity by which the approximation proceeds being the ratio $r/\{R+\sqrt{(R^2-r^2)}\}=k$, where r, $R-r$, are the radii of the mean cross-section and aperture respectively; when the ring is small this is very approximately $r/2R$. The condition that the hollow must be a free surface, gives a relation which the volume of the hollow must satisfy, which for very small rings reduces to the constancy of the radius of the hollow. For a solid ring the corresponding condition is, of course, the constancy of volume. This makes an essential difference between the two theories.

To a second approximation the velocity of translation is unaltered and is given by

$$V=\frac{\mu}{16\pi a}\left(3\log\frac{4}{k}-2\right),$$

whilst to the second approximation the surface velocity, relative to the hollow itself, is

$$U=\frac{\mu}{4\pi ak}\left(1+\frac{5}{2}k^2\right),$$

where a is the radius of the "critical" circle—or the length of a

x 2

tangent from the centre to the ring, and is therefore equal to R for small rings—and μ is the cyclic constant. For a solid ring, with the same notation,

$$V=\frac{\mu}{4\pi a}\left(\log\frac{4}{k}-2\right).$$

In the steady motion considered, the fluid carried forward with the ring forms a single mass without aperture, even for extremely small cores, though not for infinitely small ones. For values of $R/r>10^3$ there will be no aperture, whilst for less values the fluid carried forward will be ring-shaped. To a first approximation the energy due to the cyclic motion is the most important, and is the same as for a rigid ring at rest of the same size, it does not depend on the velocity of translation, except in so far as this determines the size of the aperture; as entering in this way the principal term varies inversely as the velocity of translation, and thus increases with diminished translatory motion, a result obtained by Sir W. Thomson* from general reasoning. The terms obtained by the second approximation arise from the translatory motion.

Lastly, the times of vibration in classes (2) and (4) above are determined, when the ring moves steadily. In class (2), or for fluted vibrations, the time of vibration for small rings is given very approximately by $\mu d/(2p\sqrt{n})$, d being the density, and p the pressure of the fluid at a great distance, whilst n is the number of crimpings in a section. This is the proof of the statement made above as to the independence of the temperature. In class (4) the time of pulsation is $(\mu d/2p)\sqrt{(\log 4/k)}$. As k depends on the size of the ring, and therefore on the energy, this time is not independent of the latter, but varies slowly with it. The times here given must be understood as applying to rings moving steadily; when a ring is changing its size they must be modified. The investigation of this case, and of that in which there is a core of denser matter than the surrounding fluid, I hope shortly to take up.

III. "Influence of Pressure on the Temperature of Volatilization of Solids." By WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, B.Sc. Communicated by Sir ANDREW RAMSAY, LL.D., F.R.S. Received June 5, 1883.

(Abstract.)

The experiments described in the paper were undertaken in order to ascertain whether solids have definite volatilizing points, under

* "Vortex Atoms," "Proc. Roy. Soc. Edin.," vi; "Phil. Mag.," (4), 34.

different pressures, as liquids have definite boiling points, and whether these pressures are identical with their vapour tensions.

When a liquid is heated by conduction, as for example when a flame is placed below a vessel containing liquid, till the temperature rises to its boiling point, either ebullition or superheating takes place. It would thus seem that, in the case of superheating, the surface of the liquid is not large enough to afford escape for the gaseous molecules liberated; or the convection currents are not rapid enough to convey the superheated liquid from the lower strata to the surface. If the surface is superheated, the first reason would seem to be correct; if the surface, on the other hand, is at its normal temperature, the second explanation is applicable. When ebullition takes place, the liquid increases its surface by the formation of bubbles, and as heat is totally absorbed in producing evaporation, the temperature of the liquid remains constant. If the supply of heat is increased, evaporation is also more rapid, and facility is given to more rapid evaporation, by the formation of more numerous and larger bubbles, which increase the evaporating surface.

A solid has a limited surface, which cannot be increased by formation of bubbles; and it might therefore be expected that on increasing the supply of heat, the temperature of the solid should rise, until a temperature is reached at which its rate of evaporation is equivalent to the rate at which heat is communicated to it. Reasoning thus, the existence of hot ice was maintained by Dr. Carnelley and other writers in a series of letters which appeared in "Nature" during the years 1881 and 1882.

On the other hand, a liquid in the spheroidal state presents a free surface of evaporation in every direction, and yet although exposed to radiation from a white-hot surface, its temperature does not rise even to the boiling point; and we find it impossible to raise the temperature of water above 90°, by applying heat to its surface. In such cases the surface appears to be large enough to allow vapour to escape with sufficient rapidity to prevent superheating.

If, then, the rate of evaporation at the surface of a solid is independent of the extent of that surface, but is influenced only by the rate at which heat is communicated to it, and, as in the case of liquids, by the pressure to which it is exposed, it follows that solids have definite temperatures of evaporation, or subliming points, corresponding to definite pressures, as liquids have definite boiling points.

Our experiments with water, acetic acid, benzene, naphthalene, and camphor, show that this is the case; and that with ice and camphor these pressures are sensibly the same as the maximum tensions of the vapours of these solids at corresponding temperatures. In the case of ice, the maximum temperature which can be attained under any given pressure is indicated by James Thomson's ice-steam line.

That the temperatures observed cannot be absolutely the same is evident; for there must be a certain excess of pressure in the neighbourhood of the surface of the solid in order to produce a flow of vapour from it to the surrounding space; and consequently the evaporating substance must have a higher temperature corresponding to this higher pressure. Our results, we venture to think, show that with solids, as with liquids, this difference, even when rapid evaporation is taking place, is an extremely minute one.

IV. "Note on the Establishment and First Results of Simultaneous Thermometric and Hygrometric Observations at Heights of 4 and 170 feet, and of Siemens' Electrical Thermometer at 260 feet above the ground." By G. J. Symons, F.R.S. Received June 6, 1883.

It is just a century since James Six (the inventor of the well-known Six's registering thermometer) commenced some occasional comparisons of the temperature of the air at the top and bottom of the tower of Canterbury Cathedral. We do not precisely know the position in which the instruments were placed, and, as thermometer screens had not then been invented, his observations can only be accepted as approximately correct; but as the work in which they are recorded is rather scarce, it may be well to give an analysis of the results. The observations were not consecutive, but made at various dates during 1784–92; the lower thermometer was 5 feet, and the upper one 220 feet above the ground. The daily maxima were about 1° warmer at the top during all frosty period; alike at the top and the bottom when the temperature was between 40° and 50°, and lower at the top by from 3° to 5° when the temperature was about 50°. The minima gave analogous but more marked differences. Some very severe frosts occurred while these experiments were in progress, and with bottom temperatures of $-2^{\circ}\cdot5$, $+6^{\circ}$, and $+6^{\circ}\cdot5$ respectively, the top temperatures were 15°, 17°, and 21°, showing an excess at the top of $17^{\circ}\cdot5$, 11°, and $14^{\circ}\cdot5$. In ordinary weather the excess of the top minima was not so great, but the average excess was 6°, and there was not a single night when it was colder at the top than at the bottom.

The author is not aware of any further experiments having been made upon this subject in this country until 1861, when the Rev. R. Main, F.R.S., Radcliffe Observer, had a record commenced of a Six's registering thermometer, and dry and wet bulb thermometers placed near the anemometer on the Radcliffe Observatory, Oxford. These instruments were 105 feet above the ground, and were read

about 9 A.M. in conjunction with other thermometers at 5 feet above the ground. The readings of these instruments have since that time been published *in extenso*, but the author has seen no discussion of them, and is not aware whether the thermometers have been verified or not, nor how they are mounted.

In 1868 Mr. James Glaisher, F.R.S., instituted a series of readings of the dry and wet bulb thermometers at the Royal Observatory, Greenwich, at the respective heights of 4 feet, 22 feet, and 50 feet above the ground. The observations which are published *in extenso* in the "Proceedings of the Meteorological Society," vol. v, p. 29, extend only from June 25 to August 6—a period of six weeks during the hottest part of the year. The results show that during the day hours the temperature at 4 feet was at times 7° and 8° higher, and at night 3°, 4°, and 5° lower than at 50 feet above the ground.

The subject was mentioned at the Meteorological Conference at Leipzig in 1872, and both Messrs. Wild and Scott undertook to have experiments made, with the following results:—

In 1872–74 Professor H. Wild carried out experiments with thermometers and hygrometers placed at the heights of 6 feet, 52 feet, and 86 feet above the ground on a scaffolding at the Pulkowa Observatory. ("Repertorium für Meteorologie," vol. v.)

During 1873–75 readings of maximum and minimum, and dry and wet bulb thermometers were made on behalf of the Meteorological Office at the Kew Observatory, 10 feet, and on the ornamental Pagoda in the Royal Gardens, Kew, at the respective heights of 22 feet, 69 feet, and 129 feet above the ground. Mr. R. H. Scott, F.R.S., in concluding his report on these observations ("Quarterly Weather Report," 1876, Appendix 3), says: "That so far as the evidence adduced in this paper goes, it indicates that, on the average of a considerable series of observations, the influence of height on mean thermometrical and hygrometrical results is not very great, but that on individual occasions very material differences are observed.

Lastly, Mr. G. Dines has recently communicated to the Meteorological Society ("Quarterly Journal," vol. viii, p. 189), the results of observations which he has made with thermometers placed 4 feet and 50 feet above the ground at his residence at Hersham, Walton-on-Thames. These observations, which extended from September, 1876, to September, 1878, inclusive, show that on the average the mean daily range of temperature at 50 feet was 2°·1 less than at 4 feet, the mean of the maxima at 50 feet being 1°·2 lower, and the mean of the minima 0°·9 higher than at 4 feet.

It will be seen from the above abstract that, up to the end of 1881, there was no precise information as to the form of the curve of daily temperature at considerable heights above the ground. It was known that the amplitude of the daily range was materially reduced, but the

amount of the reduction was unknown, and, owing to the absence of hourly readings at night, no data existed for determining the shape of the curve during the night and early morning hours.

The Council of the Meteorological Society have therefore thought it expedient to endeavour to obtain accurate and complete information upon the subject. The author had been for some years in communication with Mr. E. C. Hackford, verger of the parish church of Boston, Lincolnshire, the tower of which rises, quite free from any obstructions, in a very flat country, to the considerable height of 273 feet. The site was extremely suitable, and Mr. Hackford was a practised observer; he having consented to make the observations, arrangements were made for obtaining instruments. The first essential was a thermometer which could be read without climbing to the top of the tower. This requirement having been brought to the knowledge of Sir W. Siemens, F.R.S., he very kindly placed at the disposal of the Council one of his electrical thermometers.

The principle of these instruments was fully explained by Sir W. Siemens in the Bakerian Lecture, 1871, and published in a paper "On the Dependence of Electrical Resistance to Temperature," printed in vol. iii of the "Journal of the Society of Telegraph Engineers." It is therefore merely necessary to state that the thermometer consists of a spiral of insulated wire coiled round a core of wood, the whole being protected by a brass sheath. The resistance of the wire increasing with increase of temperature it is only necessary to measure the resistance in order to learn the temperature. The current is provided by a six-cell Leclanché battery, and a light cable carries the insulated wires up to the thermometer, and connects it with a galvanometer with a roller contact. The position of the contact maker when the galvanometer is at zero depends upon the resistance in the thermometer, and therefore the position of the contact maker, as shown on a graduated scale, gives the temperature.

On receipt of the thermometer from Messrs. Siemens Brothers' works, it was at once forwarded to Kew Observatory for verification, and the report was that it worked very satisfactorily, but was somewhat sluggish.

The consent of the vicar (Canon Blenkin) to the use of the tower of the church having been obtained, the Assistant Secretary of the Meteorological Society (Mr. W. Marriott) was intrusted with all the arrangements. He went to Boston at the end of February, 1882, and erected the following instruments.

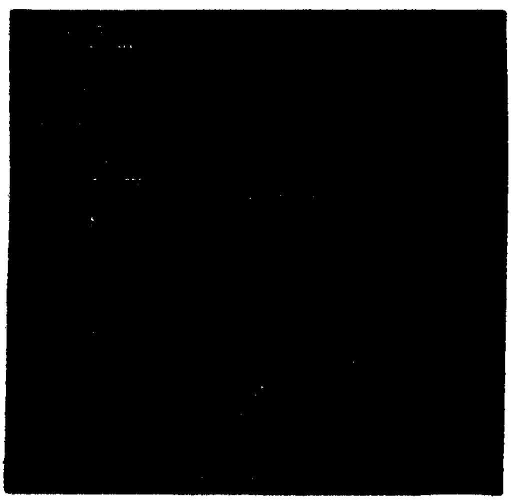
(1.) In the churchyard, where there is a good exposure, a Stevenson screen containing dry bulb, wet bulb, maximum and minimum thermometers, all at 4 feet above the ground.

(2.) On the roof of the belfry, in its south-east corner, 4 feet above the roof, and 170 feet above the ground, a precisely similar Stevenson

screen with similar thermometers to those at 4 feet. From this point upwards the tower is quite open, besides which there is free circulation of the air through the large open windows on the four sides of the tower.

(3.) In a box with three of its sides louvre boarded, and of which the height, breadth, and depth are respectively 1 foot 5 inches, $8\frac{1}{2}$ inches, and $5\frac{1}{2}$ inches, the Siemens electrical thermometer and also a standard thermometer for occasional check readings. The box or "wall screen" faces west-south-west, is attached to one of the northern pinnacles of the tower, and is 260 feet above the ground. When first put up a board was fastened on the south side of the screen to protect it from the sun's rays; as this, however, hardly afforded sufficient protection, an outer screen was placed over the wall screen in September. The cable has been brought down the inside of the tower, then through the belfry, and so to the base of the tower inside the church, where the galvanometer is fixed.

The accompanying figure shows the relative situations of the three sets of thermometers.



A = Siemens' Thermometer.
B = Stevenson's Screen on Belfry.
C = Stevenson's Screen in Churchyard.

All the thermometers are read every morning at 9 o'clock; and simultaneous readings of the electrical thermometer at the summit of the tower and of the dry bulb thermometer in the churchyard, together with the direction and force of the wind, the amount of cloud, weather, &c., are also made regularly at 11 A.M., 1, 3, 5, 7, and 9 P.M.

All the observations have been made by Mr. Hackford in the most careful and satisfactory manner.

Systematic observations commenced with April, 1882, and the whole of the records have been examined by Mr. Marriott. As the series is still in operation, and some classes of weather have not yet occurred, it is impossible to give at present final results. The leading features of the first nine months' work are, however, of considerable interest, and may be thus epitomized:—

Table I gives the monthly means of the readings of the maximum, minimum, and dry bulb thermometers in the churchyard, and on the roof of the belfry, at 9 A.M., from April to December, 1882.

From this it will be seen that the mean maximum temperature at 4 feet exceeds that at 170 feet by 1°·9, while the mean minimum temperature at 4 feet falls below that at 170 feet in almost every month, the mean difference being 0°·4. The range of temperature is, therefore, 2°·3 less on the belfry than it is at 4 feet above the ground.

The temperatures at 9 A.M. show that on the average the air at 4 feet is 1°·3 warmer than at 170 feet.

Table I.

1882.	Churchyard and Belfry.								
	Maximum.			Minimum.			9 A.M.		
	4 feet.	170 feet.	Diff.	4 feet.	170 feet.	Diff.	4 feet.	170 feet.	Diff.
April	53°·4	50°·9	−2°·5	40°·3	40°·5	+0°·2	46°·8	45°·2	−1°·6
May	60·3	57·4	−2·9	43·8	44·9	+1·1	53·4	51·2	−2·2
June	63·2	60·6	−2·6	49·0	48·9	−0·1	56·3	54·6	−1·7
July	68·4	65·4	−3·0	52·2	52·6	+0·4	60·8	58·6	−2·2
August	66·3	63·9	−2·4	52·0	52·6	+0·6	59·4	57·7	−1·7
September	61·3	59·5	−1·8	47·4	48·6	+1·2	54·3	53·1	−1·2
October	55·3	54·3	−1·0	44·8	45·2	+0·4	49·6	49·0	−0·6
November	46·7	46·0	−0·7	37·4	37·8	+0·4	41·1	41·0	−0·1
December	42·4	42·0	−0·4	34·2	34·1	−0·1	37·7	37·5	−0·2
Mean	57·5	55·6	−1·9	44·6	45·0	+0·4	51·0	49·7	−1·3

Table II gives the monthly means of the readings of the dry bulb thermometer in the churchyard at 4 feet, and of the electrical thermometer at the top of the tower, at 260 feet, above the ground, for every two hours from 9 A.M. to 9 P.M. from April to December, 1882.

From this it will readily be seen that during the period in question the mean temperature in the churchyard during these day hours is

Table II.

1882.	Churchyard and Tower.																				
	9 A.M.			11 A.M.			1 P.M.			3 P.M.			5 P.M.			7 P.M.			9 P.M.		
	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.	4 feet.	260 feet.	Diff.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	
April	46·8	44·8	−2·0	49·4	47·3	−2·1	51·1	49·5	−1·6	51·4	49·4	−2·0	50·2	49·1	−1·1	47·2	46·7	−0·5	45·4	44·9	−0·5
May	53·4	51·0	−2·4	56·1	53·3	−2·8	57·7	54·7	−3·0	57·6	54·8	−2·8	56·6	54·0	−2·6	53·8	52·2	−1·6	50·7	50·2	−0·5
June	56·3	53·6	−2·7	59·0	55·9	−3·1	60·7	57·9	−2·8	60·3	57·3	−3·0	59·6	57·0	−2·6	57·6	55·5	−2·1	54·2	52·9	−1·3
July	60·8	57·2	−3·6	63·7	60·1	−3·6	65·5	62·2	−3·3	65·8	62·7	−3·1	64·5	61·7	−2·8	61·3	59·2	−2·1	58·1	57·0	−1·1
August	59·4	56·4	−3·0	62·6	59·5	−3·1	64·1	61·2	−2·9	63·9	61·0	−2·9	62·9	60·3	−2·6	60·0	58·1	−1·9	57·7	56·3	−1·4
September............	54·3	52·2	−2·1	57·7	55·5	−2·2	59·3	57·6	−1·7	59·5	57·1	−2·4	57·7	55·9	−1·8	55·3	54·4	−0·9	53·4	53·1	−0·3
October	49·6	48·4	−1·2	52·6	51·0	−1·6	54·4	52·5	−1·9	54·1	52·3	−1·8	52·2	51·1	−1·1	50·8	50·0	−0·8	50·0	49·4	−0·6
November	41·1	40·6	−0·5	43·8	42·6	−1·2	45·0	43·6	−1·4	44·8	43·6	−1·2	43·1	42·5	−0·6	42·1	41·6	−0·5	41·7	41·5	−0·2
December	37·7	37·5	−0·2	38·9	38·1	−0·8	40·4	39·2	−1·2	40·2	39·6	−0·6	39·3	39·1	−0·2	38·9	38·1	−0·8	38·5	37·6	−0·9
Mean	51·0	49·1	−1·9	53·8	51·5	−2·3	55·4	53·2	−2·2	55·3	53·1	−2·2	54·0	52·3	−1·7	51·9	50·6	−1·3	50·0	49·2	−0·8

Table III.

1882.	Churchyard and Tower.																	
	9 P.M.			10 P.M.			11 P.M.			Midnight.			1 A.M.			2 A.M.		
	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
November 11–12	35·2	38·7	+3·5	34·8	36·6	+1·8	30·9	36·5	+5·6	30·5	37·2	+6·8	31·0	37·4	+6·4	33·3	37·4	+4·1
" 18–19	37·2	35·3	−1·9	37·2	34·9	−2·3	37·2	36·1	−1·1	38·2	37·4	−0·8	38·6	38·0	−0·6	39·1	39·9	+0·8
" 25–26	40·3	38·4	−1·9	39·3	38·4	−0·9	39·3	38·6	−0·7	38·4	38·4	0·0	38·3	38·0	−0·3	38·2	38·0	−0·2
December 2–3	34·1	31·9	−2·2	34·9	33·2	−1·7	34·3	33·3	−1·0	35·2	32·7	−2·5	34·0	32·3	−1·7	34·0	32·3	−1·7
" 9–10	34·6	33·4	−0·9	35·0	34·1	−0·9	34·1	34·1	0·0	34·1	34·1	0·0	33·6	32·9	−0·7	33·3	33·3	0·0
" 16–17	42·8	41·0	−1·8	42·5	41·8	−0·7	43·1	42·5	−0·6	43·2	42·5	−0·7	43·3	42·5	−0·8	43·4	42·5	−0·9
" 23–24	31·3	32·9	+1·6	33·2	34·9	+1·7	33·2	34·9	+1·7	32·9	34·6	+1·7	32·1	34·0	+1·9	32·4	34·0	+1·6
" 30–31	38·5	37·1	−1·4	38·6	36·7	−1·9	38·2	37·4	−0·8	38·2	37·4	−0·8	38·5	38·0	−0·5	40·2	38·7	−1·5

1882.	Churchyard and Tower.																	
	3 A.M.			4 A.M.			5 A.M.			6 A.M.			7 A.M.			8 A.M.		
	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
November 12	33·0	38·0	+5·0	31·1	35·8	+4·7	30·7	35·0	+4·3	31·1	34·2	+3·1	30·3	34·2	+3·9	30·1	33·6	+3·5
" 19	40·7	40·7	0·0	42·3	43·1	+0·8	42·3	42·3	0·0	42·3	42·3	0·0	38·2	39·9	+1·7	38·4	39·9	+1·5
" 26	39·1	38·0	−1·1	39·3	39·9	+0·6	40·3	39·9	−0·4	40·2	39·7	−0·5	40·2	39·9	−0·3	40·3	39·8	−0·5
December 3	34·0	32·3	−1·7	34·1	32·9	−1·2	35·0	34·5	−0·5	35·5	34·7	−0·8	35·7	35·1	−0·6	(36·5)	(35·5)	−1·0
" 10	33·3	33·3	0·0	33·5	33·3	−0·2	33·5	33·5	0·0	33·1	33·5	+0·4	32·7	31·8	−0·9	30·5	31·1	+0·6
" 17	43·6	43·1	−0·5	44·3	43·7	−0·6	44·4	43·7	−0·7	44·3	43·7	−0·6	44·3	43·7	−0·6	44·7	43·7	−1·0
" 24	32·2	33·8	+1·6	32·1	33·6	+1·5	31·5	33·6	+2·1	31·2	33·6	+2·4	31·2	32·3	+1·1	31·1	32·9	+1·8
" 31	40·3	38·7	−1·6	40·3	39·2	−1·1	40·5	39·3	−1·2	40·7	39·3	−1·4	40·9	39·3	−1·6	41·3	39·3	−2·0

Table III—(continued).

1882.	Churchyard and Tower.																				
	9 A.M.			10 A.M.			11 A.M.			Noon.			1 P.M.			2 P.M.			3 P.M.		
	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.
November 12	° 31·6	° 36·0	° +4·4	° 33·6	° 38·0	° +4·4	° 37·2	° 41·4	° +4·2	° 40·6	° 43·7	° +3·1	° 43·9	° 44·1	° +0·2	° 44·9	° 43·8	° −1·1	° 44·8	° 43·7	° −1·1
" 19	40·3	38·9	−1·4	40·5	38·3	−2·2	41·3	39·8	−1·5	42·3	40·8	−1·5	40·5	38·9	−1·6	41·1	42·3	+1·2	40·7	40·2	−0·5
" 26	40·6	39·4	−1·2	42·0	39·7	−2·3	43·2	41·6	−1·6	42·3	40·3	−2·0	42·7	40·0	−2·7	42·5	40·0	−2·5	43·9	42·3	−1·6
December 3	37·4	36·0	−1·4	(39·0)	(37·5)	−1·5	40·3	38·7	−1·6	40·9	39·3	−1·6	41·7	42·8	+1·1	42·7	43·2	+0·5	43·5	45·0	+1·5
" 10	32·5	32·3	−0·2	33·5	32·1	−1·4	34·4	32·3	−2·1	34·1	31·1	−3·0	34·5	32·8	−1·7	34·3	32·5	−1·8	34·1	32·9	−1·2
" 17	45·3	43·8	−1·5	45·9	44·5	−1·4	46·3	44·7	−1·6	47·3	44·7	−2·6	47·2	45·4	−1·8	47·1	45·1	−2·0	47·1	44·8	−2·3
" 24	30·3	32·3	+2·0	32·1	32·6	+0·5	33·1	32·7	−0·4	35·7	33·2	−2·5	36·2	35·3	−0·9	(35·9)	(35·3)	−0·6	35·6	35·3	−0·3
" 31	42·5	40·4	−2·1	42·9	41·2	−1·7	44·2	42·5	−1·7	45·3	43·2	−2·1	46·5	44·6	−1·9	47·3	46·3	−1·0	47·5	46·7	−0·8

1882.	Churchyard and Tower.																	
	4 P.M.			5 P.M.			6 P.M.			7 P.M.			8 P.M.			9 P.M.		
	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.	4 ft.	260 ft.	Diff.
November 12	° 43·3	° 43·3	° 0·0	° 41·3	° 43·6	° +2·3	° 40·3	° 43·3	° +3·0	° 40·1	° 41·9	° +1·8	° 40·3	° 41·8	° +1·5	° 38·5	° 42·1	° +3·6
" 19	38·2	37·3	−0·9	38·2	37·9	−0·3	37·7	37·1	−0·6	36·4	36·7	+0·3	36·3	35·5	−0·8	36·1	34·5	−1·5
" 26	44·3	42·5	−1·8	43·3	42·5	−0·8	42·3	41·8	−0·5	43·3	42·1	−1·2	43·3	41·9	−1·4	43·2	41·6	−1·6
December 3	43·3	45·5	+2·2	43·5	43·2	−0·3	43·2	42·7	−0·5	43·6)	(42·9)	−0·7	44·0	43·1	−0·9	44·3	43·1	−1·2
" 10	34·1	32·1	−2·0	34·0	31·9	−2·1	33·1	31·0	−2·1	32·6	31·0	−1·6	32·3	30·5	−1·8	32·1	31·0	−1·1
" 17	44·9	44·7	−0·2	44·5	44·6	+0·1	44·9	43·1	−1·8	44·9	43·1	−1·8	43·8	41·8	−2·0	(43·5)	(41·5)	−2·0
" 24	34·7	35·3	+0·6	34·3	35·5	+1·2	34·6	33·8	−0·8	34·5	33·3	−1·2	34·7	34·0	−0·7	(34·9)	(34·0	−0·9
" 31	47·5	46·2	−1·3	46·9	45·7	−1·2	47·1	44·6	−2·5	46·9	45·0	−1·9	46·7	45·2	−1·5	46·9	45·0	−1·9

considerably warmer than at the top of the tower. The greatest difference is from 9 A.M. to 3 P.M., after which it begins to get less and less.

On examining the individual readings for each day, two interesting features are clearly brought out, viz., (1) that in fine bright weather the temperature at 4 feet during the day is much higher than on the top of the tower,—in summer the difference sometimes reaches 5° at 1 P.M.; and (2) that in foggy weather the temperature at the top of the tower is always higher than at 4 feet, the upper part of the tower being generally free from fog. In cloudy and wet weather the temperature is uniformly higher in the churchyard than at the top of the tower.

These observations, however, like nearly all those previously made, refer mostly to the day time; it was felt of pre-eminent importance that readings should be taken occasionally throughout the night as well as the day. Hitherto the usual difficulties in obtaining observations requiring night attendance have prevented any considerable number of such records being obtained, but every effort will be made to increase their number, especially when extreme atmospheric conditions prevail. During last winter there were eight occasions upon which one of the church officials was on duty all night, and for those periods we have complete records.

The following are brief descriptions of the weather on each occasion:—

November 11–12. Fine clear cold night, followed by a fine day, with slight fog.
,, 18–19. Dull, wet and squally.
,, 25–26. Fine bright night, dull day, rather windy.
December 2–3. Dull, with snow at night, fog in afternoon.
,, 9–10. Dull and cold.
,, 16–17. Dull and damp.
,, 23–24. Fine and bright, slight fog 5–9 P.M.
,, 30–31. Cloudy night, wet day with fog.

The temperatures are given in Table III.

The prevalence of fine clear nights is readily seen by the increased temperature at the upper station; the presence of fog is also indicated in a similar manner; while cloudy skies, rain, and wind prevent radiation, and so on the days and nights of their occurrence the temperature is always highest near the ground.

From the foregoing it seems that the difference between the temperature at 4 feet and 260 feet is chiefly regulated by the amount of cloud, and by the relation of the temperature of the surface of the ground to that of the general body of air passing over it. If so, it will follow that the mean difference between the temperature at the two heights can only be determined by very numerous observations,

or by careful considerations of the conditions of weather and of soil temperature under which each individual set is made.

As regards hygrometry the data at present obtained relate solely to 9 A.M. and to the heights of 4 and 170 feet respectively. The mean relative humidity at these points for nine months of 1882, is given in the following table, which shows that the differences, though somewhat larger in summer than at other periods, are not generally of importance.

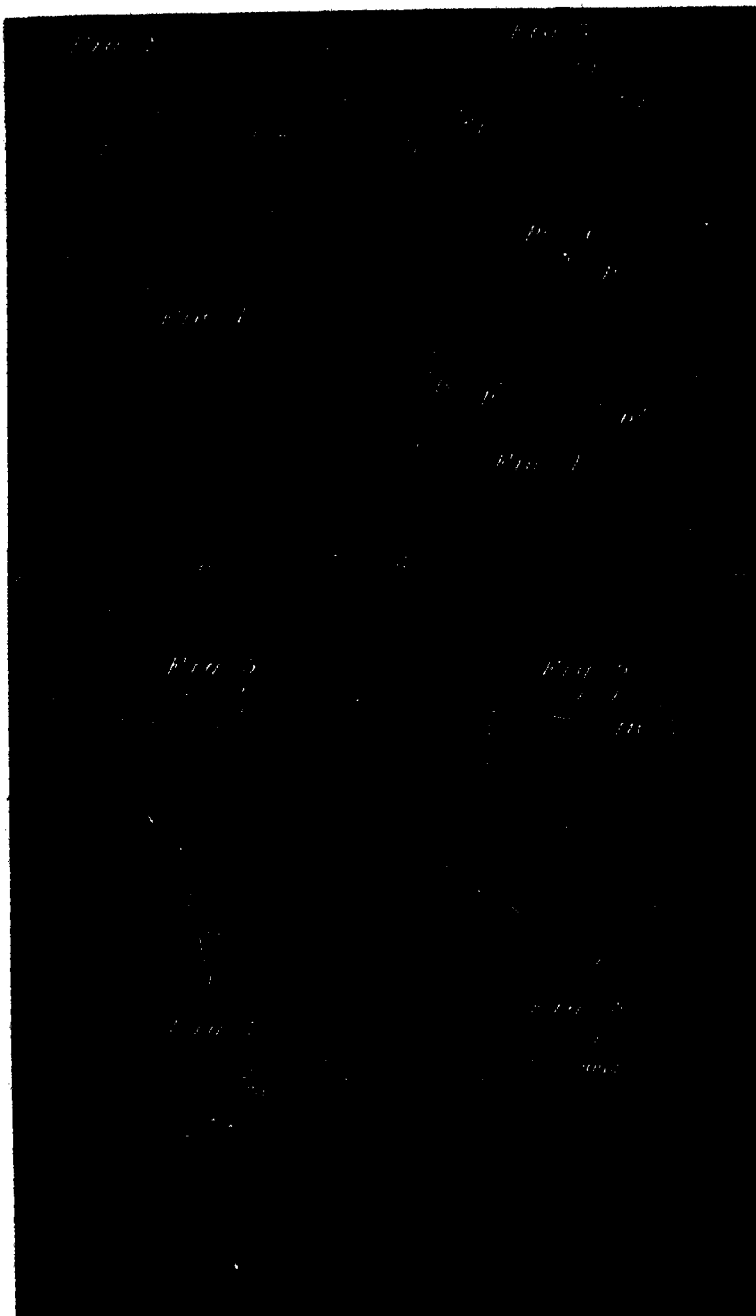
1882.	Relative humidity.		
	4 feet.	170 feet.	Difference.
	Per cent.	Per cent.	Per cent.
April	84	80	−4
May	75	77	+2
June	79	78	−1
July	73	78	+5
August	78	83	+5
September	85	88	+3
October	90	93	+3
November	87	90	+3
December	96	98	+2
Mean	83	85	+2

V. "On Curves circumscribing Rotating Polygons with reference to the Shape of Drilled Holes." By A. MALLOCK. Communicated by Lord RAYLEIGH, M.A., F.R.S. Received June 11, 1883.

It is well known that drills and other tools of the same class which are guided by their cutting edges tend to, or at any rate can produce holes which are not circular in section, and further, that if the tool have n equidistant edges, the hole which it produces, if not circular in section, will have $n+1$ sides or similar arcs,* but the exact shape of these holes and the limits to their possible departure from circularity have not as far as I am aware been hitherto examined.

The problem consists in finding all the figures which will circum-

* These sides are in general very much curved, and in fact are not distinguishable without measurement when n is greater than 3; but when $n=2$ the departure from the circular form is often very wide. A good instance of this may be seen in the holes bored in rock for blasting. These holes are made by an iron bar with a chisel-shaped end, and the section of the holes is a triangle with the corners rounded off. Fig. 7.



scribe a rotating polygon, that is, the figures on whose sides the angles of the enclosed polygon shall all lie during the whole revolution of the latter in the former. But before proceeding to do this it may be as well to consider how the departure from the circular form originates.

Let AB, CD, fig. 1, be the tool, which for simplicity may be supposed to have two cutting edges only, AC, BC; its axis being CD.

The tool is guided by the edges AC, BC, and the axis may be considered to remain parallel to a fixed straight line, but to be otherwise free.

In fig. 2 let DEF be the section of the hole, ABC the drill seen in plan, and C′ the axis of the hole.

If the hole is circular C and C′ will be identical, but if by any chance C and C′ are mutually displaced, one edge (BC in the figure) will have a deeper layer of substance to cut than the other, and the result will be that the instantaneous axis about which the drill is revolving is shifted from C to some point, P, lying between C and B, but by turning about any point between C and B, AC will have to cut through a layer of substance gradually increasing in depth, and when this depth is equal to that along BC, P and C will coincide, but as BC will now encounter a layer of diminishing thickness, P will move through C to some position between A and C.

These alternate variations of the depth of the cut at the two edges may continue indefinitely, the section of the hole varying at each revolution, but if certain conditions are fulfilled the figure produced will remain constant in character after the first revolution.

Since P is always on that side of C on which the cut is deepest, and C is on the same side of the perpendicular from C′ on AB, C will describe some curve round C′ in the opposite direction to the rotation of ACB. That is, while the drill rotates in one direction, its geometrical axis describes some curve in the opposite direction round the axis of the hole.

If instead of the centres C and C′ being mutually displaced, one of the edges meets with an accidental obstruction, or if one of the edges is blunter than the other, the same results will be produced, except that when the last mentioned cause operates the effects tend to accumulate, and this probably is the actual origin in most cases.

The same sort of result will occur whatever be the number of the cutting edges of the tool, viz., the instantaneous axis will always be on that side of the geometrical axis on which the cut is deepest, or the resistance to progress of the edge greatest, and in virtue of this the geometrical axis will rotate about the axis of the hole in a direction opposite to the motion of the tool.

This fact at once suggests a method of analysing the figures swept

out by the angles of the tool. Let ϵ be the distance between C and C′ and θ the direction of the line joining them, then ϵ can always be expressed by the following equation :—

$$\epsilon = \epsilon_0 + \Sigma\epsilon_i \sin(i\theta + \alpha_i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

and since the tool is a rigid body, its angles are displaced from the position which they would occupy were the hole circular by an amount ϵ in a direction θ.

Let A_1, A_2 (fig. 3), be the adjacent angles of the tool, which is supposed to be a regular polygon of n sides, P_1, P_2 the paths along which they move, c the centre of hole, and p_1 p_2 the path described by c', the centre of the polygon.

It is clear that if $P_1, P_2, \ldots$ are to form parts of a single curve, $\ldots A_3$, A_2 must occupy the same position when the polygon has turned through $\frac{2\pi}{n}$ that $\ldots A_2$, A_1 occupied at first; thus, while the polygon rotates once, its centre will move n times round the centre of the hole, and hence if ϕ be the angle which AC′ makes with a fixed line passing through c, and if θ also be measured from the same line,

$$-n\phi = \theta,$$

$$\therefore \epsilon = \epsilon_0 + \Sigma\epsilon_i \sin(in\phi + \alpha_i) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2);$$

hence ϵ goes through n complete cycles, while ϕ increases by 2π; but as ϵ revolves in a direction opposite to that of the polygon, it will go through $n+1$ cycles if the θ be measured from a line fixed in the polygon. Let CA be this fixed line, then A goes through $n+1$ cycles of displacement in virtue of the first term of ϵ, viz., ϵ_0, and $in+1$ cycles in virtue of the ith term, while CA revolves once; hence the symmetry of the curve $P_1, P_2, \ldots$ will be $n+1$-fold as far as it depends on ϵ_0; but the only other terms which will give symmetrical curves if present with ϵ_0 are those for which i has such values that while ϕ changes from $\frac{2\pi}{n+1}$ to $\frac{2\pi}{n}$, $in\phi$ changes through $\frac{2\pi}{n+1}+$ a complete multiple of 2π, or for which

$$i = 1 + p(n+1),$$

where p is an integer, and if the symmetry of $P_1, P_2, \ldots$ is to be complete—that is, if each interval, $P_1\,P_2$, $P_2\,P_3$, &c., is to be composed of similar halves—α_i must be either 0 or a multiple of π.

If the sum of the coefficients ϵ_i in ϵ is small compared with CA, the equation of the curve P_1P_2 referred to its centre is approximately

$$\rho = \rho_0 + \epsilon\cos(n+1)\phi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

where $\quad \rho = \mathrm{C'A}$ and $\rho_0 = \mathrm{CA}$.

If, however, without confining ourselves to necessarily small values of ϵ, we consider only the first term ϵ_1, which is always the most important in practice, the curve $P_1P_2 \ldots$ is a hypotrochoid having for its Cartesian coordinates

$$y=\rho_0 \sin\phi-\epsilon_0 \sin n\phi,$$
$$x=\rho_0 \cos\phi+\epsilon_0 \cos n\phi,$$

where, if a and b are the radii of the rolling circles,

$$a=\rho_0\frac{n-1}{n},$$

$$b=\frac{\rho_0}{n}.$$

Thus we see that a hypotrochoid with its generating circles in the ratios given will circumscribe an n-sided rotating polygon, the diameter of whose circumscribing circle is $2\rho_0$. In order that the rotation may be mechanically possible, the polygon and hypotrochoid must not cut one another at any part of the revolution, and this condition limits the possible value of ϵ.

Let ABCD, fig. 4, be part of the polygon, and EFGH part of the hypotrochoid, then it may be shown that if a side of one figure cuts that of the other, the cutting sides will contain the greatest area between them when, as at B and C, the adjacent angles of the polygon are equidistant from the adjacent corners of the hypotrochoid. For the angular motion of B and C is a maximum when passing the middle of the side of the hypotrochoid, and a minimum when passing the corners, so that, since BF=CG, C will leave G faster than B approaches F, and *vice versâ*, hence the area enclosed between the points p_1, p_2, by BC and FG will diminish if the polygon rotates, and hence if BC is a tangent to FG when BF=CG, the sides can never cut one another. The distance of the middle point of the side of the polygon from its centre is $\rho_0 \cos\frac{\pi}{n}$, while the distance of the corresponding point of the hypotrochoid is $\rho_0-\epsilon$, and equating these quantities, we get as the greatest admissible value of ϵ,

$$\epsilon=\rho_0 \operatorname{vers}\frac{\pi}{n}.$$

If the hypotrochoid is to be everywhere concave to the centre, ϵ must not be greater than $\frac{\rho_0}{n^2}$.

The following table shows the values of ϵ in the two cases from $n=2$ to $n=7$:—

n.		$\epsilon = \frac{\rho_0}{2}$ vers $\frac{\pi}{n}$.		$\epsilon = \frac{\rho_0}{n^2}$.
2	......	$\rho_0 \times \cdot 5$	......	$\rho_0 \times \cdot 25$
3	......	·25	......	·115
4	......	·146	......	·062
5	......	·095	......	·040
6	......	·067	......	·027
7	......	·051	......	·020

Figs. 5 and 6 show the hypotrochoids corresponding to the values of ϵ in the first column, and figs. 7 and 8 those in the second, for $n=2$ and $n=4$.

There are some propositions relating to the form of cylindrical turned surfaces which have an analogy to that considered in this paper. These I hope to examine in a future communication.

VI. "Contributions to our Knowledge of the Connexion between Chemical Constitution, Physiological Action, and Antagonism." By T. LAUDER BRUNTON, M.D., F.R.S., and J. THEODORE CASH, M.D. Received June 13, 1883.

(Abstract.)

This paper is divided into several parts. In the first the authors consider the general action of ammonium salts, and show that the action of the ammonium is considerably modified by the acid radical with which it is combined.

All the ammonium salts affect the spinal cord, motor nerves, and muscles, and in advanced poisoning tend to poison all those structures. The course of poisoning varies with the salt employed. The chloride, bromide, and iodide form a series in which a stimulant action on the cord is best marked in the bromide, and the paralysing action upon it and upon motor nerves most strongly in the iodide.

Motor nerves are also paralysed by the sulphate and phosphate, though less strongly than by the iodide.

The iodide tends to arrest the circulation sooner than the other salts.

Ammonium bromide appears to have a special tendency to cause coagulation of the stroma of red blood corpuscles. The sulphate does so to a less extent, and the phosphate and iodide have a still less effect.

In the second part the action of salts of compound ammonias was investigated. The substances employed were ethylamine, trimethylamine, and triethylamine; the chlorides, iodides, and sulphates of amyl

ammonium and of dimethyl ammonium; trimethyl ammonium and tetramethyl ammonium iodide; ethyl ammonium chloride, diethyl ammonium chloride, iodide, and sulphate; triethyl ammonium chloride, iodide, and sulphate; tetraethyl ammonium iodide.

The compound ammonias affect the spinal cord, motor nerves, and muscles. The spinal cord is first stimulated and then paralysed, stimulation being evidenced by twitchings or convulsions, and the paralysis by loss of reflex action and motor power.

There is a marked difference in the action between ammonia and the compound ammonias; while ammonia causes well marked *tetanus, compound ammonias,* as a rule, *produce symptoms of motor* paralysis with the exception of those in which one atom of hydrogen only is substituted by an alcohol radical. This paralysis appears to be partly due to their action on the spinal cord and nerve centres, and partly to a curara-like action on the motor nerves.

Some of them apparently increase somewhat the excitability of the spinal cord at first, but this is temporary, and is shown rather by hyperæsthesia or tremor than by convulsion; and tetramethyl and ethyl ammonium salts differ from the di- or tri-methyl or ethyl ammonias in having a much greater tendency to cause convulsions.

The effect of the acid radical on the physiological action is less marked in the case of the compound ammonias than in the salts of ammonia itself.

The iodides of the compound ammonias paralyse motor nerves more quickly than either chlorides or sulphates.

No difference was observed between the paralysing action of corresponding chlorides and sulphates.

The irritability of the muscle is increased as a rule by the chlorides, sometimes increased and sometimes diminished by the sulphates, and, as a rule, though with some exceptions, decreased by the iodides. The *contractile* power of the muscle is less affected by the chlorides, more by the sulphates, and most by the iodides.

On comparing the effect of the substitution of different alcohol radicals for hydrogen in the compound ammonias, it was found that the least active substances were the ethyl, diethyl, and triethyl compounds. In the case of the iodides and sulphates of these compounds only, was the minimal irritability of the muscle undiminished. The difference between ethyl and methyl compounds was more observable in the case of the iodides and sulphates than in that of the chlorides. The iodides all have a strong tendency to paralyse the motor nerves, but this is most marked in the case of tetramethyl and tetraethyl ammonium iodides. Tetramethyl appears to act in a somewhat smaller dose than the tetraethyl ammonium.

Sulphates of the methyl compounds have a stronger tendency to paralyse motor nerves than those of ethyl compounds, and the ethyl

compounds have a greater tendency to exaggerate the irritability of the muscle at first.

Salts of methyl, ethyl, and amyl ammonium are more active than the corresponding ones of the di- and tri-compounds, but the tetra-compounds are most active of all.

In the next part of the paper the effect of salts of the alkalies on muscle and nerve are considered. The substances investigated were the *chlorides* of lithium, sodium, potassium, rubidium, and cæsium. These differ from ammonia in having very little tendency to stimulate the spinal cord, and the chief symptom of poisoning by them is increasing torpor.

Slight excitement of reflex action is noted at first in the case of potassium and rubidium.

The motor nerves are not paralysed by cæsium or rubidium except in very large doses, but the other substances of this group paralyse them to a greater or less extent. Lithium and potassium are the most powerful.

The contractile power of muscle (as shown by the height of curve) is increased by rubidium, ammonium, potassium, and cæsium. It is unaffected by sodium excepting in large doses, and is almost invariably diminished by lithium.

The *duration of contraction*, as shown by the length of the curve, is increased by rubidium and ammonium in large doses, by sodium, and cæsium. It is shortened by ammonium, lithium, and rubidium in small doses, and by potassium. The contracture or viscosity is increased by rubidium and ammonium in large doses, by lithium, and sodium. It is diminished by rubidium and ammonium in small doses, by cæsium, and potassium.

The action of substances belonging to the alkaline earths and earths, is discussed in the next section. The substances investigated were the chlorides of calcium, strontium, barium, beryllium, didymium, erbium, and lanthanum. In regard to their action upon the nervous system, these substances fall into two groups, (*a*) containing beryllium, calcium, strontium, and barium; and (*b*) containing yttrium, didymium, erbium, and lanthanum. Group *a* has a tendency to increase reflex action as evidenced by spasm or tremor. With group *b* reflex action in the cord appears to be little affected, but they appear to have a tendency to paralyse motor centres of the brain in the frog.

Group *a* all paralyse motor nerves to some extent. Lanthanum has also a slight paralysing action, but the other members of group *b* have not, agreeing in this respect with sodium and rubidium, and differing from all the others.

In regard to their action on muscles these substances cannot be divided into sub-groups. The *contractile* power of muscle is increased by barium, erbium, and lanthanum. It is sometimes increased and

sometimes diminished by yttrium and calcium. It is diminished by didymium, strontium, and beryllium. The duration of contraction is increased by barium, calcium, strontium, yttrium, and erbium. It is unaffected, or slightly diminished by beryllium, didymium, and lanthanum.

Contracture is increased by barium, calcium, strontium, yttrium, and beryllium. The *contracture* produced by barium is enormous, resembling that produced by veratria, as the authors have shown in a former paper. It is, like that of veratria, diminished by heat, cold and potash, and may be abolished by these agents. It is not so well marked when the drug is injected into the circulation as when locally applied to the muscle.

The action of some of the more important of those drugs can be graphically represented by a spiral, the terminal members of which are potassium and barium, and these two are to a certain extent connected by ammonium as an intermediate link.

The alteration effected in the action of the different members of these groups on muscle by the subsequent application of another, is next discussed, and it is shown that the effect of one substance upon muscle may be increased or diminished by the application of another. One of the most curious points is that two substances having a similar action may, instead of increasing, neutralise each other's effect.

Potassium shortens the lengthened muscular curves produced by barium, calcium, strontium, sodium in large doses, and lithium, and reduces the *contracture* which they cause. Sodium in large doses lengthens the curve and increases the *contracture* of the normal muscular curve, and it adds to the length of the long curves caused by calcium and strontium. The veratria-like curve of barium is counteracted by almost all the substances which produce a shorter curve than itself. There is remarkable antagonism between barium and rubidium. Rubidium in large doses produces an elongated curve with enormous *contracture* almost like that of barium. This abnormal curve is reduced to the normal by barium, and if this is applied to a greater extent than is sufficient to antagonise rubidium, it again produces the prolonged curve characteristic of the barium itself. In the case of calcium and strontium, which have a similar action in prolonging the curve, we find no antagonism; the one tending rather to increase the effect of the other. Some relations are pointed out between the atomic weights of antagonising elements. Although the data are too limited to draw from them any general rule, the authors think that they may possibly lead by-and-by to some useful result. Thus, calcium reduces the barium curve to the normal before it causes its own peculiar curve. This may be looked upon possibly as the result of the union of the barium and potassium having

resulted in some compound which is a multiple of potassium; potassium being, as we know, an important constituent of normal muscle.

$$\begin{array}{ll} \text{Ba } 137\times 2=274 & 274-\text{Ca } 40=234, \\ \text{K } \ 39\times 6= & 234. \end{array}$$

Rubidium in large doses has the same effect as barium in causing a veratria-like curve, but barium destroys the effect of rubidium before producing its own effect.

$$\begin{array}{l} \text{Rb } 85{\cdot}4\times 8=683{\cdot}2, \\ \text{Ba } \ 137\times 5=685. \end{array}$$

In the next division the authors show that by alternate application of acids and alkalies the muscle of the frog may be made to describe on a slowly-revolving cylinder curves which almost exactly resemble those described on a quick cylinder by the normal contraction of a muscle on stimulation; and also those which the muscle describes on irritation after it has been poisoned by barium. They consider that the contraction of muscle may be possibly due in some measure at least to alterations in acid or neutral salts which the muscle contains.

The lethal activity on frogs of the chlorides experimented upon is as follows: the potassium being most powerful, and calcium least powerful; potassium, beryllium, rubidium, barium, ammonium, cæsium, lithium, lanthanum, didymium, erbium, strontium, yttrium, sodium, calcium.

VII. "The Influence of Water in the Atmosphere on the Solar Spectrum and Solar Temperature." By Captain ABNEY, R.E., F.R.S., and Colonel FESTING, R.E. Communicated at the request of the Committee on Solar Physics. Received June 14, 1883.

In our paper on "Atmospheric Absorption in the Infra-red of the Solar Spectrum" ("Proc. Roy. Soc.," vol. 35, p. 80), we stated that the absorption by water coincided with the absorption bands to be found in the solar spectrum, and our proof rested on photographs which had been taken for some time back. In the diagram which we published (and in which are slight errors in shading at some parts, and which we here correct) we showed the coincidences as far as λ10,000, that being the limit to which we could accurately fix the wave-lengths. Simultaneously almost with the publication of our paper, there came into our possession an account of Professor Langley's researches on the solar spectrum, conducted by means of the bolometer, a perusal of which determined us to vary

our method of observations in order to definitively test our conclusions as regards intensity of absorption in the part of the spectrum below the red which we had explored, and also to ascertain if our deductions held good in the parts we had not explored. It thus became necessary to go over the work done by Langley as far as our instrumental means would allow. One of the most remarkable features in it seemed to be the failure of Cauchy's formula for refraction, his prismatic spectrum extending below A to a distance equal to AG, whilst theoretically it should end at a distance equal to AE. Our first labour was to ascertain the correctness of this by means of photographs taken with the diffraction grating. Since 1880, when the temporary map of the normal spectrum of the infra-red from λ7000 to 10,500 was published, a large number of photographs for the determination of lower wave-lengths have been taken, with the result that a map as far as 15,000 can now be constructed, showing all the delicate Fraunhofer lines which exist in this lower region, but this extent is not sufficient for a comparison with Langley's solar thermogram, which extends to λ28,000. Unfortunately in our climate the atmospheric conditions preclude getting any results much lower than 15,000, except on very rare occasions. Advantage has been taken of cold dry days to take rougher photographs, which though not so defined in detail as those up to λ15,000, are yet sufficiently accurate to compare with Langley's map. His map is accurate to figures over the 100 on the tenth-metre scale, and our photographs have the same accuracy. As a result of the comparison of our photographs to λ22,000, with the map, we may say, that the wave-lengths agree, and the failure of Cauchy's formula is confirmed. Our photographs were secured by separating vertically the different orders by means of a white glass prism of 30°. The coincidences of the different spectra were thus readily seen, and the λ value at once ascertained. The glass employed throughout our researches is the same as that used by Langley, to whom one of us recommended it, and of which he speaks in such high praise. The use of rock-salt for anything except for purely absolute quantitative work becomes unnecessary, and for comparative work the glass is as effective and more certain. No rock-salt prism which we have tried will bear the use of a collimator, the lens of which is filled by the radiation; the surfaces have never been nearly as perfect as those of the glass. The principal Fraunhofer lines were almost indistinguishable, unless the aperture of the collimating lens was very largely diminished. This was the case with four separate prisms, which when first tried were newly polished by an optician. Professor Langley has been more fortunate than ourselves in this respect, and has been able to compare his glass with rock-salt. We owe, however, a debt of gratitude to Dr. Guthrie and Professor Tyndall for having given us rock-salt with which to experiment, and

only wish our report of its behaviour might have been more favourable.

Having confirmed the correctness of Langley's wave-lengths as far as $\lambda 22,000$, we proceeded by means of the thermopile to examine the spectrum obtained when using the crater of the positive pole of the electric light as a source of radiation, and also subsequently the variations caused by placing different thicknesses of water in front of the slit. The thermopile we used was constructed specially for us by Elliott Brothers, some two years ago, when we had made a commencement of this work.* It is a linear pile of twenty-six couples to the inch, and one face is covered by a silvered slit with moveable jaws. The couples are mounted in the usual manner, and enclosed in a chamber with transparent ends which can be exhausted, if necessary. Outside this again is a water jacket, through which a constant stream of cold water can be made to circulate. The instrument is extremely delicate, and answered our purpose well. Two reflecting galvanometers were used at different times, one having an internal resistance of about 0·5 ohm, and the other of 0·12 ohm. The thermopile was mounted on a stand, moveable horizontally by a screw of $18\frac{1}{2}$ turns to the inch. The range of movement was 6 inches. The pile was always moved in one direction during one set of observations. To form the spectrum the following arrangement was adopted. A condenser of white glass threw an image of the crater on the slit of a collimator having a focal length of 20 inches, the lens of the collimator had a diameter of $1\frac{1}{4}$ inches, and the rays of light were rendered parallel for C, and then fell on a glass prism (sometimes two) placed at approximately the angle of minimum deviation for that ray. A lens of 20 inches focal length attached to a camera formed the spectrum, which could be received either on a sensitive plate or on the face of the pile. The width of the collimator slit was $\frac{1}{80}$ of an inch for the electric lights, and for the sun $\frac{1}{240}$ inch. The width of the slit of the thermopile was in the former case $\frac{1}{60}$ inch, and in the latter $\frac{1}{75}$. In order to obtain fiducial points in the spectrum, the arc was focussed on the slit, a sodium spectrum formed, and a photograph taken. The sensitive plate was replaced by the thermopile, and the D line brought in the centre of the jaws of the silvered slit, and this point on the screw was used as the zero of the scale. From measurements of the photographs the exact position of all lines and bands could be readily obtained and referred to the reading taken with the thermopile. A check photograph and zero reading were taken at the end of each series of observations. An assistant kept the image of the crater on the slit (which had a length of ·31 inch), one of us attended to the movement of the pile, while the other took the galvanometer readings. The unused face of the pile was covered with thick folds of non-con-

* "Nature," vol. 25, 1881–82.

ducting material, and the jacket of the pile was kept full of water, the whole being enclosed to form part of the camera by means of a thick covering. At each movement of the pile at least three readings of the galvanometer were taken. In each series of observations, and for each water spectrum examined, at least six series were made.

We believe that the results we put forward are fairly representative quantitatively, and without doubt the positions of maxima and minima may be relied on to one-eighth turn of the screw. At the same time it must be remembered that the breadth of the slit of the thermopile is a *measurable quantity*, and one which cannot be neglected, but its existence is a necessary evil. For this reason the positions of bands are more exact in the photographs than in the thermograms, and any small discrepancy in position should be discounted in favour of the photograph. When using the solar radiation, which is four times more intense than that of the crater of the electric light, and when both slits have been narrowed, the absorptions are sure to be more intensely marked than when a wider slit is used, the reason of which is obvious. Langley employed a spectrum which was of about the same purity as ours ("American Journal of Science," vol. 25, p. 169, 1883, and "Phil. Mag.," vol. 15, p. 153, 1883), but the width of his bolometer strip being about one-fifth that of the jaws of the thermopile slit, and his spectrum longer, the absorptions are shown with greater strength than they are in our curves, whilst in our photographs the absorptions are even more marked.

Our first result was the thermogram of the crater of the positive pole, and this is shown in Curve I, Diagram I. This is the mean of a dozen curves which all lie *very closely* on this mean. One thing we learn from it is that the temperature of the crater is always very uniform with any carbon, and is what we might expect when we consider that it is the temperature at which carbon vaporises. In comparing it with Tyndall's curve given in "Heat as a Mode of Motion," we are struck by the difference exhibited in the two near the apices; this difference, however, is explicable as will be seen further on, when it is remembered that his source of radiation was one composed of various temperatures. Next, in order to ascertain the amount of absorption due to different thicknesses of water, it was found necessary to employ cells with glass sides, and, unfortunately, not being in possession of parallel glass of the same material as the prisms, it became necessary to take the absorption curve of the cells. This is found in Curve II. Water was then placed in the cells before the slit, and the readings taken. Curves VI, VII, and VIII are the water curves corrected for the cell-absorption, and they were constructed to enable a comparison to be made with the curve of the solar spectrum.

Annexed are tables showing the ordinates of the different points of the spectra as determined by the thermopile.

Curves. (See Diagram I.)

Scale No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	
−10	·5	·4	·4	·4	0	·5	·5	0	
− 8	1·3	·9	·9	·9	0	1·3	1·3	0	
− 6	2·5	1·3	1·3	1·3	0	2·5	2·5	0	
− 4	4·4	2·5	2·5	2·3	·2	4·4	4·1	·3	
− 2	5·9	4·5	4·3	4·1	1·2	5·5	5·0	2·0	
0	11·0	7·5	7·3	6·8	3·2	10·5	9·5	4·7	This point is D.
½	12·3	8·7	8·7	8·5	4·0	12·3	12·0	5·7	
1	13·7	10 0	9·2	8·8	3·7	13·0	12·3	5·3	
1½	15·1	11·3	10·7	10·0	5·0	14·0	13·3	—	
2	16·8	12·8	12·2	11·6	6·7	16·0	15·0	—	
3	21·5	16·0	15·9	15·0	10·3	21·4	20·0	—	
3¼	·23·0	16·7	16·7	16·0	10·7	23·0	22·5	15·3	
3½	24·2	17·5	17·5	16·7	8·5	24·2	23·0	—	
4	27·5	19·3	18·5	17·0	2·3	26·0	24·0	3·5	This point is A.
4½	31·5	21·1	21·0	19·2	1·8	31·5	28·0	—	
5	35·0	22·8	22·8	20·6	1·4	35·0	32·2	2·0	
5½	38·2	24·6	23·0	21·0	—	36·0	33·0	—	
5¾	40·0	25·4	24·7	22·0	—	38·7	34·5	—	
6	41·2	26·2	24·3	20·0	0	38·0	27·0	0	
6¼	42·3	27·0	23·0	14·0	Spectrum ends.	—	—	Spectrum ends.	
6½	43·3	27·7	21·5	5·3		33·7	8·5		
6¾	44·5	28·4	24·0	6·0		—	—		
7	45·3	29·3	26·0	9·0		—	—		
7¼	46·5	30·0	26·3	10·0		40·2	15·5		
7½	45·7	29·4	25·0	9·7		—	—		
8	43·2	27·6	20·5	1·5		32·2	—		The minimum probably lies a little closer to 8 than 8¼.
8¼	—	—	18·5	·5		28·0	·8		
8½	40·0	25·8	18·5	·5		—	—		
8¾	—	—	19·0	·7		—	—		
9	37·3	24·0	16·0	·5		—	—		
9½	34·3	21·4	6·0	0		9·7	0		
10	31·3	18·5	·7	0		1·3	Spectrum ends.		
10½	27·3	15·0	1·5	Spectrum ends.		—			
11	24·0	11·6	3·0			5·3			
11½	—	—	1·6			—			
12	15·5	6·8	1·0			—			
12½	—	—	0			0			
13	9·0	4·0	·4			—			
13½	—	—	1·5			2·8			
14	3·7	1·8	1·1			—			
15	1·8	·8	0			0			
18	0	0							

A reference to the diagram will show that the thermograms of the water spectra have absolute coincidence with the absorptions shown in the photographs. The thermogram is, however, more delicate in some respects; as for instance, in the dip in the curves commencing at ½ in the scale. This is a water-band which is only shown photographically and markedly with 4 feet of water, whilst with but

$\frac{1}{8}$ inch of water it is distinct in the thermogram. The reason of this is that when high intensities of radiation are nearly equal, an apparent equality of intensity of image is produced in a photograph when exposure is prolonged. By shortening the exposure and thus sacrificing the rest of the spectrum, this band can be brought out. Russell and Lapraik (see "Journal of the Chemical Society," 1881) found this band visually when using a thickness of 14 feet of water, apparently, therefore, the thermopile is more delicate for gentle shades of radiation than is the eye.

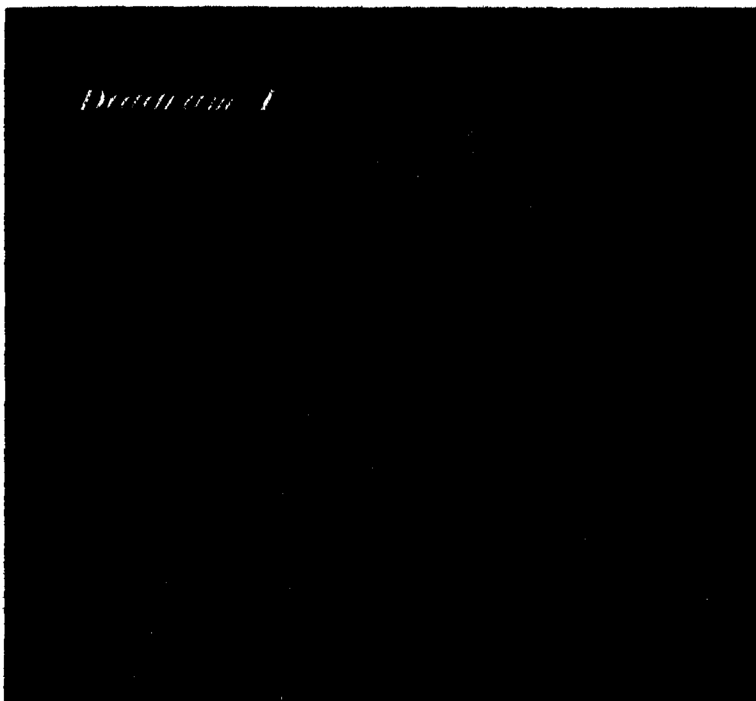
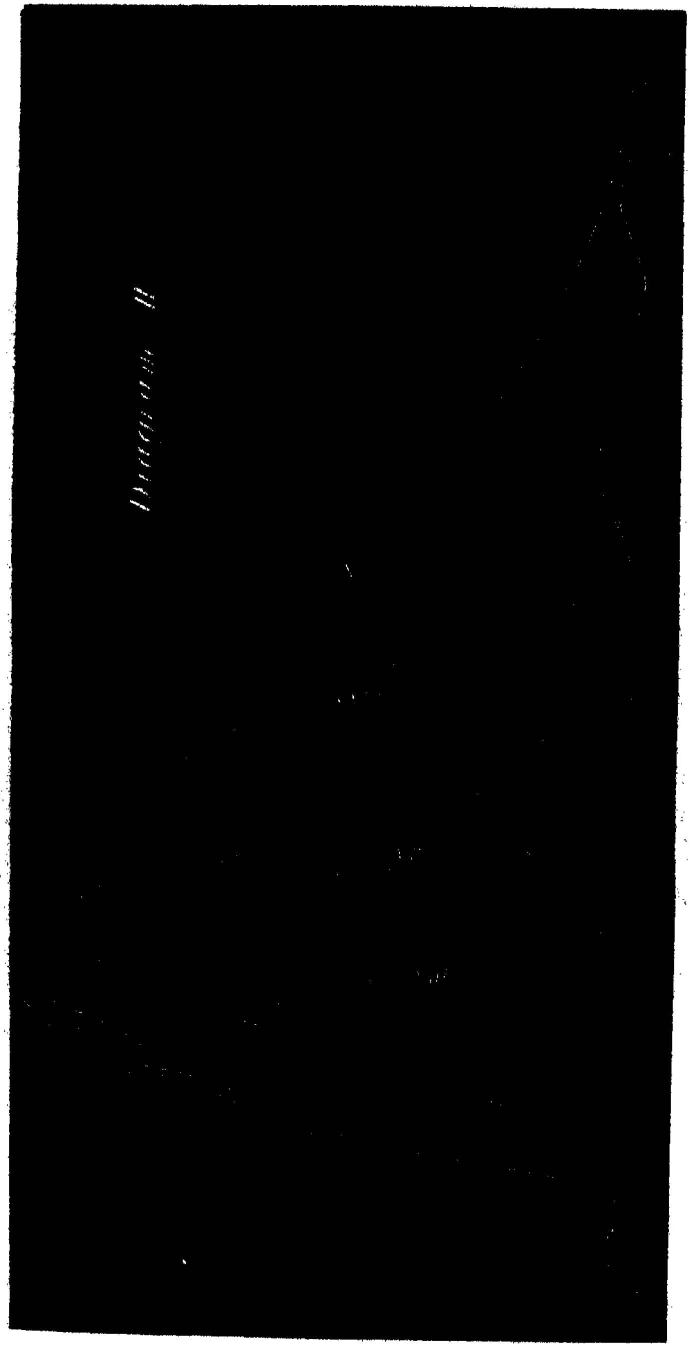
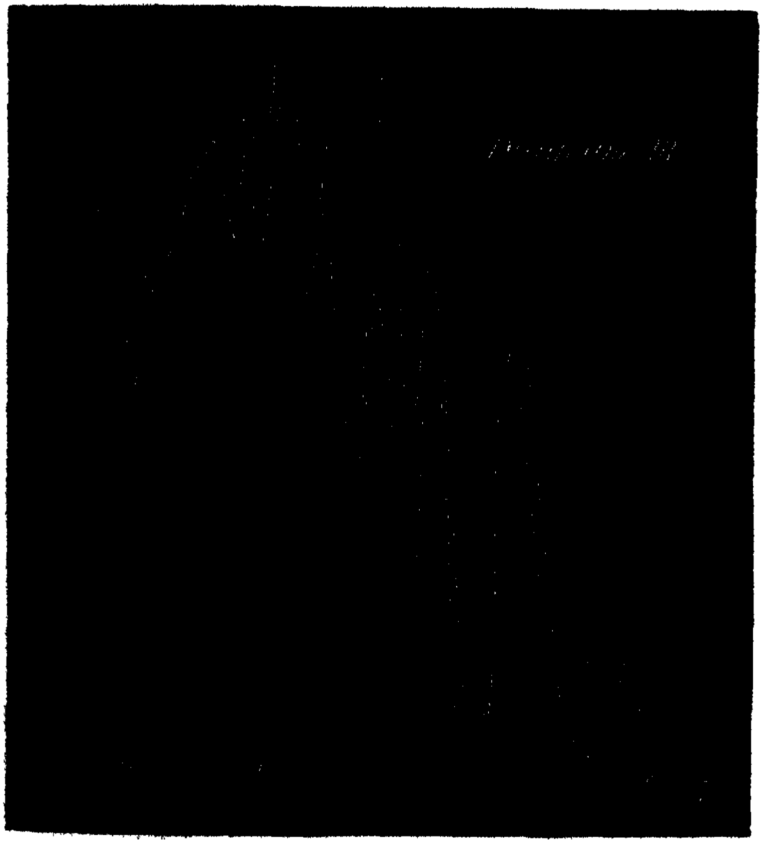


Diagram II shows the Curves I, VI, VII, and VIII, transformed to the normal scale of wave-lengths (figured I′, VI′, VII′, and VIII′ respectively), from which it will be seen that the maximum energy of the crater on this scale corresponds to a wave-length of about 7350, or just at the limit of the red as usually visible. It will also be seen that the depressions are not so marked as in the prismatic scale. Diagram III gives the solar spectrum on two separate days. The continuous curve was taken with the wet and dry bulb of the thermometer standing at 68° and 78° F. respectively. According to usually accepted methods of calculation, this would correspond to an equiva-

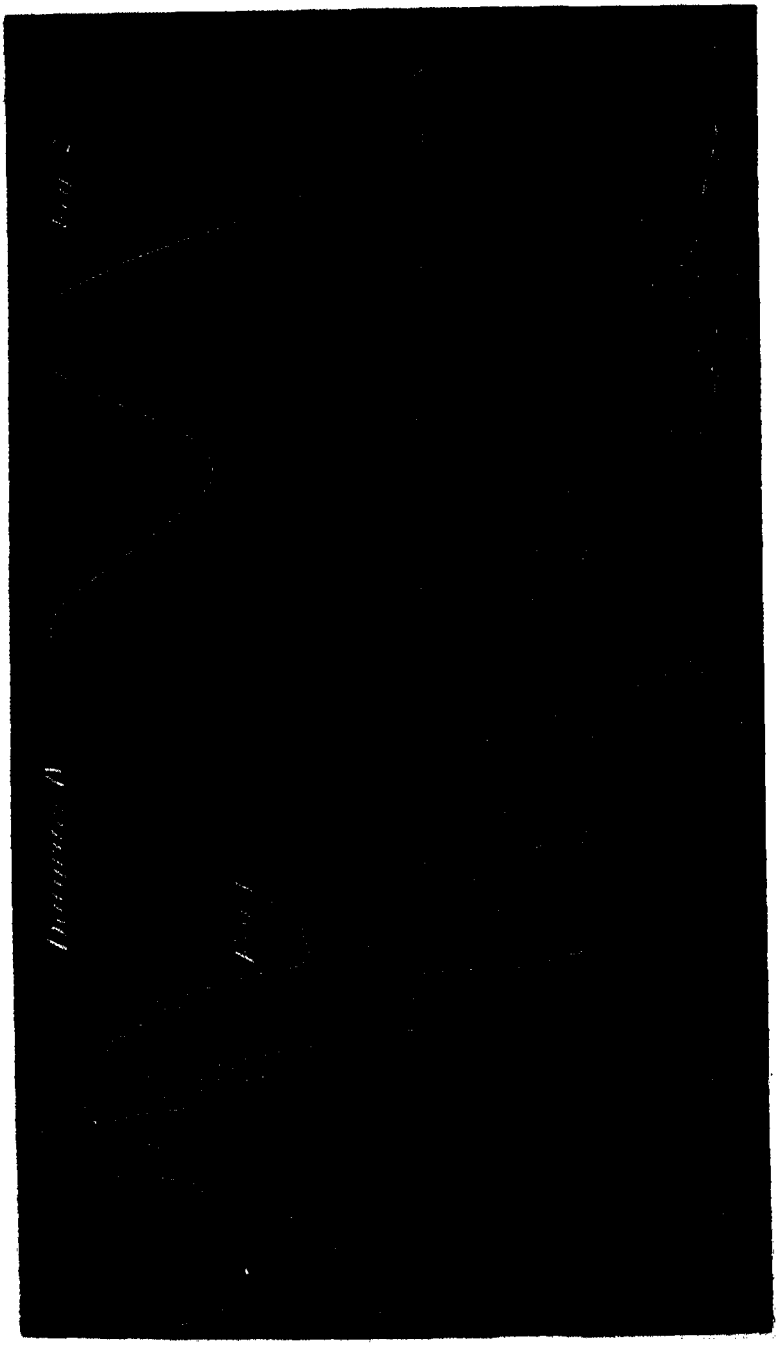


lent of about 5½ inches of water between the sun and the spectroscope. The dotted curve was taken on a day which was drier, but on which the sky was a little hazy.



The following is a table of the ordinates of the continuous curve of Diagram III

Scale No	Readings	Scale No.	Readings
0	18	3·5	35
0½	20	3·75	38·5
1	26	4	34
1½	28·3	4¼	34
2	31	4½	41
2½	36·5	4¾	40
2¾	37	5	34·5
3	35·5	5⅓	29·5
3·3	33	5·5	29



Scale No.	Readings.	Scale No.	Readings.
5·75	34	$9\frac{1}{2}$	7
$6\frac{1}{3}$	19	10	2
$6\frac{2}{3}$	18	$10\frac{1}{2}$	22
7	28	11	25
$7\frac{1}{5}$	38	$11\frac{1}{2}$	16
$7\frac{1}{2}$	35	$12\frac{3}{5}$	0·5
$7\frac{3}{4}$	30	$13\frac{1}{10}$	7
8	20	$13\frac{3}{5}$	4·5
$8\frac{1}{5}$	19	$14\frac{1}{10}$	8
$8\frac{2}{5}$	25	$14\frac{3}{5}$	0
$8\frac{3}{5}$	28	15	1
9	21 5	16	0

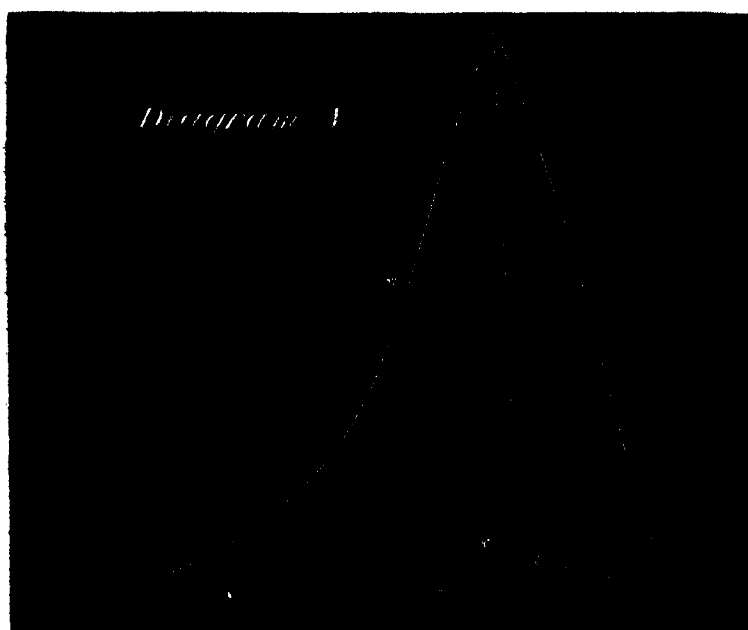
Diagram IV shows the continuous curve of Diagram III transferred to the normal scale, and that in dotted line is Langley's curve ("Phil Mag," March, 1883) transferred to our scale, so that the maxima correspond The chain-dotted curve is curve No. VI', Diagram II; but it must be borne in mind that the vertical scales are different, inasmuch as the slits of the thermopile and collimator were narrower in the case of the sun than in the case of the electric light. To make an absolute comparison, the ordinates of the solar curve must be multiplied by 4·5 It will be seen that, generally, our curve corresponds with Langley's, the difference in position of the maxima and minima being in no case greater than one-sixth turn of the screw of the thermopile. His radiation receiver was so much narrower than ours, that his readings would show the rises and depressions more exactly. A comparison of the curves will show that almost all the atmospheric absorption is due to watery stuff. The depression shown in Langley's and our own solar curves at 8660 is not coincident with water. His shows the water-band at 8240 (which is that shown in the photographs), and, no doubt, we missed it owing to the steepness of the descent from λ7900. The dip at 8660 is probably due to a hydrocarbon, to which we will not here refer further in the present communication. We wish to emphasise the fact that the "a" group, corresponding to λ7200, is not due to water as a liquid; nor when we look at Diagram I and see the absorption of 2 feet of water, can we suppose that in the dip from 7300 to 7600 the A line is included.

Professor Langley in his paper states that the atmospheric absorption in the infra-red of the spectrum is comparatively small, and he gives a hypothetical thermogram of the extra-atmospheric solar spectrum in which the maximum energy is in the yellow, and the energy curve descends steadily on both sides of this maximum. We do not think our experiments quite confirm his views in this respect.

We submit that a comparison of the thermograms of the solar spectrum and those of the electric light points to the conclusion that much the greater portion of the absorption in the infra-red of the former is due to the presence of water-stuff. In the case of the electric light it will be seen how large a proportion of the energy is absorbed by even $1\frac{3}{4}$ inches of water, especially between $\lambda 10,000$ and $\lambda 21,000$. If water in the state of vapour, as we believe to be the case, absorbs as much energy as the same amount in a liquid state, the sun's rays are absorbed to at least a corresponding extent in passing through our atmosphere, which must, we believe, always contain more than an equivalent of $1\frac{3}{4}$ inches of water.

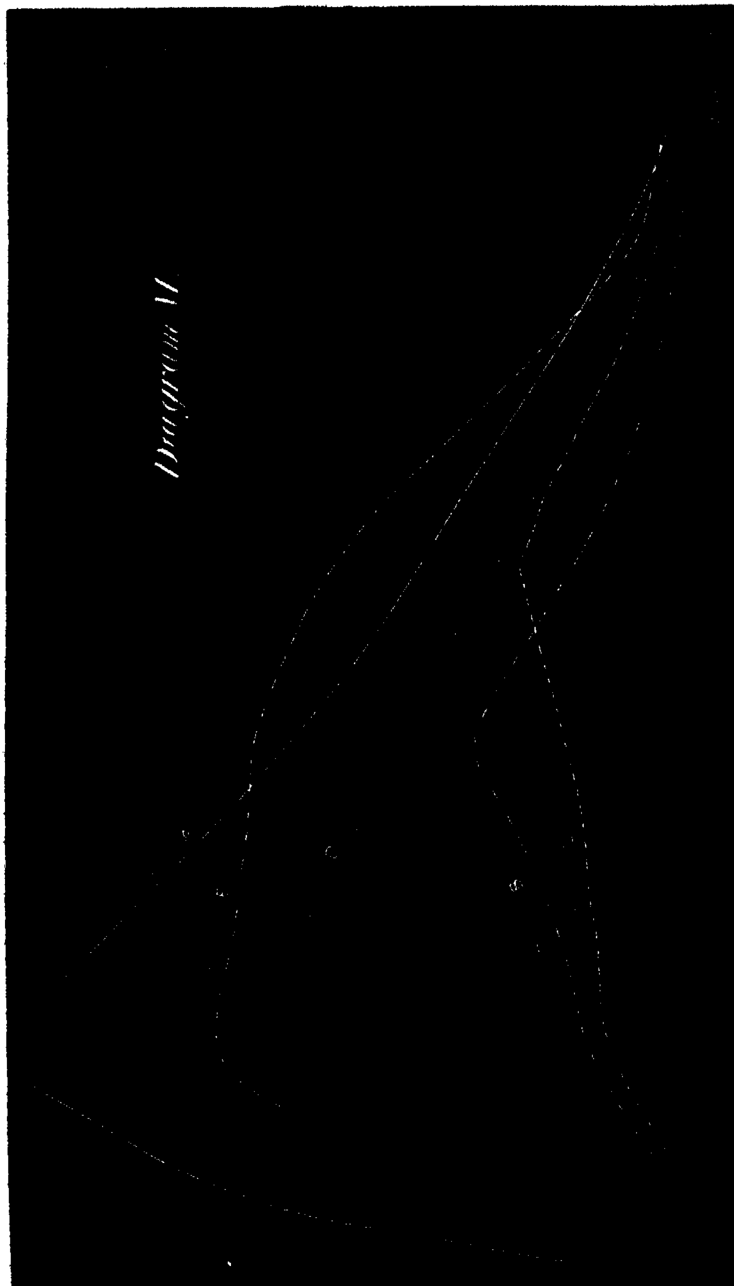
Now Langley's normal solar curve may be considered representative of a thermogram taken under the most favourable conditions for dryness of atmosphere, and it cannot be supposed, even under these circumstances, that between the spectroscope and the solar atmosphere there could have been less than $\frac{1}{8}$ inch of water (or its equivalent in vapour), judging by the depression in it. Fig. 2, Diagram IV, shows the general form of his curve corrected for the absorption due to $\frac{1}{8}$ inch of water; from which it appears that there must be a large proportion of energy emanating from a source of comparatively low temperature.

The prismatic thermogram of the spectrum from a source having a



single temperature appears to be one having a sharply-defined maximum (see Diagram V). Curve A is the curve of the crater of the electric light, Curve B is that of an incandescent light produced with thirty-eight Grove's cells, and Curve C that produced by thirty Grove's cells; the same lamp being used in both cases. The current used was 2·25 ampères and the electromotive force 5·1 volts in the last case, and in the former 2·95 ampères and 6·3 volts. (The deflections of the galvanometer attached to the thermopile, due to the naked lights, in the two cases were as 105 to 207.) The thermogram from a source of mixed temperatures would be the integration of those from each temperature separately. In illustration of this we have taken the three thermograms just mentioned, and transformed them (see Diagram VI) to the normal scale of wave-lengths, the ordinates of B being exaggerated four times, and those of C eight times. Curve D is formed by making the ordinates the mean between those of A and B, and the ordinates of E are the sums of those of D and C. Thus E would represent the thermogram of a source composed of the temperature of the positive pole of an arc lamp, an incandescent lamp worked by thirty-eight cells and one worked by thirty cells, in the proportion of 1, 4, and 16. In these last two curves, there is a lump, so to speak, which corresponds approximately in position to that shown in Langley's solar curve as corrected for $\frac{1}{8}$ inch of water (see fig. 2, Diagram IV), indicating the presence of energy due to a low temperature source or sources. The mottling on the sun's disk when seen in a telescope makes it evident that at its visible surface there is a considerable range of temperature, and we have evidence of eclipse observations that outside its ordinarily visible surface there is much matter at a still lower temperature which is competent to produce a continuous solar spectrum. With these amongst other facts before us, we are disposed to doubt whether the extra-atmospheric spectrum of the sun would give such a simple thermogram as Professor Langley has suggested, and whether the maximum energy would be found in the position in which he places it. We believe that the curve would have a form rather more allied to our Curve E.

At first sight it might appear hopeless to try and affix an approximate value to any solar temperatures, but we think that it is not impossible to determine with some approach to probability the maximum temperature to which any compound curve is partially due, more particularly when, as in the case before us, the general form of the corrected curve indicates an excess in quantity of low temperature. From an inspection of the curves in Diagram V it appears that for any temperature higher than that of Curve A the position of the maximum will be but very slightly shifted towards the more refrangible end of the spectrum, also that the general form of the curve must be similar to that of A, and that the areas within the curves, which



are measures of energy, will be very nearly proportional to the ordinates of the maximum or to ordinates not far from them on the more refrangible side.

Now the part of the spectrum which suffers least absorption by water, and which in the solar spectrum is free from any very intense Fraunhofer lines, is near 4 on the prismatic scale. It has been shown by Dewar,* and he deduced the same from Rosetti's formula, that the temperature of the source is nearly proportional to the square root of the total radiation, or, in other words, of the area of the thermogram curve. The incandescent lamp worked by thirty cells had approximately a temperature of 1,100°, and the same when worked by thirty-eight cells of 1,500°, and the areas of the curves in the two cases are as 106·4 to 50·4, and the galvanometer deflections when compared together, as already stated, as 105 to 207, or about two to one in both cases. This would give about the temperatures above stated, taking either one as correct. The area of the curve of the crater is very nearly sixteen times that of the thirty-eight cell curve, or the temperature of the crater, using Dewar's formula, would be about 6,000°, a temperature corresponding to that obtained by him.

We have already shown at what point of the solar spectrum aqueous absorption has least effect, and if we may calculate from this the probable area of the curve of maximum temperature. In our prismatic solar thermogram the scale of the ordinates is 4·5 times the scale of the crater thermogram, but the ordinates of the point of the spectrum above referred to are nearly equal. Hence the highest solar temperature would be $\sqrt{4\cdot5} \times 6000°$, or about 12,700°, using the same relations of temperature and radiation.† Taking, however, the square root of the whole area of the solar curve, as would be the case when direct and not spectrum measurements were made, we should get a temperature of about $\sqrt{2\cdot5}$ times that of the crater, or 9,600°, a temperature also very similar to that obtained by Dewar under the conditions specified.

In conclusion we say, 1st, that the thermopile experiments have confirmed our previous views as to the coincidence of the absorptions by water and those shown in the solar spectrum; 2nd, that the extension of the work beyond our previous point emphasises them; 3rd, that the highest temperature of the sun is not less than 12,700°, a temperature far higher than that which has been recently put forward by Sir W. Siemens; 4th, that the existence of a large quantity of solar radiation due to low temperature has been shown to be more than probable.

* "Brit. Assoc. Rep.," 1878, p. 465.

† Since this paper was read we have made further experiments in regard to this relation, and believe that we may have to modify it, reducing the temperature somewhat.—July 29.

VIII. "Supplement to former Paper entitled—'Experimental Inquiry into the Composition of some of the Animals Fed and Slaughtered as Human Food,'—Composition of the Ash of the Entire Animals, and of certain Separated Parts." By Sir JOHN BENNET LAWES, Bart., LL.D., F.R.S., F.C.S., and JOSEPH HENRY GILBERT, Ph.D., LL.D., F.R.S., V.P.C.S. Received June 11, 1883.

(Abstract.)

In a former paper ("Phil. Trans.," Part II, 1859) the authors had given the actual weights, and the percentage proportion in the entire body, of the individual organs, and of certain more arbitrarily separated parts, of 326 animals—oxen, sheep, and pigs—in different conditions as to age, maturity, fatness, &c. They called particular attention to the wide difference in the proportion by weight of the stomachs and intestines in the three descriptions of animal; the proportion of stomach and contents being very much the highest in oxen, considerably less in sheep, and little more than one-tenth as much in pigs as in oxen. On the other hand, the intestines and contents contributed a less proportion to the weight of the body in oxen than in either sheep or pigs; the percentage by weight in pigs being nearly twice as high as in sheep, and more than twice as high as in oxen. With these very characteristic differences in the proportion of the receptacles and first laboratories of the food, the other internal organs collectively, as also the blood, contributed a pretty equal proportion by weight of the entire body, in the three descriptions of animal.

Ten animals had been selected for the determination of the chemical composition, namely—a fat calf, a half-fat ox, and a fat ox; a fat lamb, a store sheep, a half-fat sheep, a fat sheep, and a very fat sheep; a store pig, and a fat pig. In these, in the collective carcass parts, in the collective offal parts, and in the entire bodies, the total nitrogenous substance, the total fat, the total mineral matter, the total dry substance, and the water, were determined; and the results were recorded and discussed in detail.

It was shown that, as the animal fattened, the percentage of nitrogenous substance decreased considerably, whilst that of the fat and of the total dry matter increased in a much greater degree. It was estimated that the portions of well fattened animals which would be consumed as human food would contain three, four, and even more times as much fat as dry nitrogenous substance; and comparing such animal food with wheat-flour bread, it was concluded that, taking into consideration the much higher capacity for oxidation of a given weight

of fat than of starch, such animal food contributed a much higher proportion of non-nitrogenous substance, reckoned as starch, to one of nitrogenous substance than bread. In fact the introduction of our staple animal foods, to supplement our otherwise mainly farinaceous diet, did not increase, but reduced, the relation of the flesh-forming material to the respiratory and fat-forming capacity of the food.

Finally, the actual amount, and the percentage, of total ash, in most of the internal organs, and some other separated parts, were given. It was shown that the percentage of total mineral matter, like that of the nitrogenous substance, decreased, not only in the entire body, but especially in the collective carcass parts, as the animals matured. It was the object of the present communication to record the results of the complete analysis of the ashes of the collective carcass parts, of the collective offal parts, and of all parts, of each of the ten animals. Forty complete ash analyses had been made.

As was to be expected, more than four-fifths of the ashes consisted of phosphoric acid, lime, and magnesia; these making up the largest amount in the ash of the oxen, less in that of sheep, and less still in that of pigs. Potash and soda were also prominent constituents. Assuming, for the purposes of illustration merely, that one of phosphoric acid was combined with three of fixed base, the ashes of the ruminants showed an excess of base; whereas, according to the same mode of calculation, the ashes of the pigs showed no such excess.

It was, unfortunately, only in the case of the offal parts of the pigs that the ash of the chiefly bony, and that of the chiefly soft parts, had been analysed separately. The results showed a considerable excess of acid, especially phosphoric, in the ash of the non-bony portions; presumably, in part at any rate, due to the oxidation of phosphorus in the incineration. In further reference to the point in question, it may be stated that although the oxen and sheep show a higher percentage of total nitrogenous substance than the pigs, yet the amount of pure ash yielded from the non-bony parts is higher in proportion to that from the bones in the case of the pigs than in that of the ruminants.

Comparing the percentage composition of the ashes of the entire bodies of the different animals, the chief points of distinction were that —in the ash of the pigs there is a lower percentage of lime, and a higher percentage of potash and soda, than in the corresponding ash of the ruminants; there is a somewhat higher percentage of phosphoric acid in the ash of the pigs and of the oxen than in that of the sheep; and there is a higher percentage of sulphuric acid (and somewhat of chlorine also) in the ash of the pigs than in that of the other animals.

A table showing the quantities of total ash, and of each individual mineral constituent, in each of the ten animals analysed was given. Not much stress was laid on the amounts in the particular animals

analysed; as the actual weights and condition of animals coming under similar designations may vary considerably.

It was of more interest to consider the amounts of the mineral constituents in carcass parts, in offal parts, and in all parts, per 1,000 lbs. fasted live-weight, of each description of animal.

It was shown that a given live-weight of oxen carried off much more mineral matter than the same weight of sheep, and a given weight of sheep much more than the same weight of pigs. With each description of animal the amounts of phosphoric acid, lime, and magnesia, are less in a given live-weight of the fatter than of the comparable leaner individuals. Of both potash and soda, again, the quantity is less in a given live-weight of the fatter animals. The same may be said of the sulphuric acid and the chlorine; in fact, in a greater or less degree, of every one of the mineral constituents.

It was estimated that the loss to the farm of mineral constituents by the production and sale of mere fattening increase was very small. It was greater of course in the case of growing than of only fattening animals. In illustration, the amounts of some of the most important mineral constituents removed annually from an acre of fair average pasture and arable land in various products were compared. Such estimates could obviously be only approximate, and the quantities will vary considerably. With this reservation, it may be stated that, of phosphoric acid, an acre would lose more in milk, and four or five times as much in wheat or barley grain, or in hay, as in the fattening increase of oxen or sheep. Of lime, the land would lose about twice as much in the animal increase as in milk, or in wheat or barley grain; but perhaps not more than one-tenth as much as in hay. Of potash, again, an acre would yield only a fraction of a pound in animal increase, six or eight times as much in milk, twenty or thirty times as much in wheat or barley grain, and more than 100 times as much in hay.

From the point of view of the physiologist, it would doubtless have been desirable that the selection of parts for the preparation and analysis of the ash should have been different, and more detailed. The agricultural aspects of the subject had, however, necessarily influenced the course of the inquiry; and the extent of the essential work had enforced the limitation which had been adopted. The results must be accepted as a substantial contribution to the chemical statistics of the feeding of the animals of the farm for human food.

IX. "On the Solubility of Salts in Water at High Temperatures."

By WILLIAM A. TILDEN, D.Sc. Lond., F.R.S., Professor of Chemistry in the Mason Science College, Birmingham, and W. A. SHENSTONE, F.I.C., F.C.S., Lecturer on Chemistry in Clifton College, Bristol. Received June 19, 1883.

(Abstract.)

This paper contains an account of experiments made with the view of determining the solubility of salts in water at temperatures above the boiling point of water. They were originally undertaken with the object of further investigating the anomalies presented by sulphate of soda, but the method adopted was afterwards applied to many other metallic salts, and the paper presents an account of determinations so made, together with a discussion of the theoretical bearing of the results.

The main conclusion arrived at is that solubility is directly related to fusibility. Of the salts operated upon some habitually crystallise with water of crystallisation, others habitually in the anhydrous state. When the latter are written down in the order of their melting points, beginning with the most fusible, it is observed that increase of solubility consequent upon a given rise of temperature above 100° C., is greatest in the most fusible, least in the least fusible, and all the cases observed follow this rule in regular order. If the results are represented graphically, taking for abscissæ the degrees of temperature and for ordinates the quantity of salt dissolved in 100 parts of water, it is at once seen that the higher the melting point the more nearly do the curves so constructed approach a straight line. The relation is illustrated by the case of the chloride, bromide, and iodide of potassium, the solubilities of which at all observed temperatures follow the order of the melting points. Also in comparing together two such salts as chlorate and chloride of potassium, the solubilities and melting points of which are as follows, the curves cutting each other at 100° :—

	Melting point.	Solubility at			
		0°.	100°.	130°.	180°.
Potassium chlorate...........	359°	3·3	56·5	88·5	190
„ chloride...........	734°	29·2	56·5	66	78

As to sulphate of sodium the solubility increases as the temperature

rises from 0° to 34°, the melting point of the decahydrated salt $Na_2SO_4.10H_2O$. Thereafter the solubility diminishes till the temperature of 120° is reached. From 120° to 140° we find the change if any is inappreciable, but at 160° a notable increase of solubility is observed, which is still further increased at 180° and at 230°, the highest temperature reached.

Parts by weight of anhydrous sulphate of sodium dissolved by 100 parts of water at							
0°.	34°.	100°.	120°.	140°.	160°.	180°.	230°.
5	78·8	42·7	41·95	42·00	42·9	44·25	46·4

In view of these and other facts, theories of hydration can no longer be admitted as competent to explain the act of solution in all or even more than a few cases.

X. "On the Determination of the Number of Electrostatic Units in the Electromagnetic Unit of Electricity." By J. J. THOMSON, M.A., Fellow and Assistant Lecturer of Trinity College, Cambridge. Communicated by Lord RAYLEIGH, F.R.S. Received June 19, 1883.

(Abstract.)

This paper contains an account of some experiments which have been made during the last two years in the Cavendish Laboratory, Cambridge. These experiments were made to determine "v" by comparing the electrostatic and electromagnetic measures of the capacity of a condenser. The condenser consisted of two cylinders fitted with guard-ring pieces. The electrostatic measure of the capacity was calculated from the dimensions of this condenser. The electromagnetic measure of the capacity was determined by a very slight modification of the method given in § 775 of Maxwell's "Electricity and Magnetism." In this method the condenser has to be repeatedly charged and discharged by a commutator, and a very elaborate commutator would be required to work the guard-ring part of the condenser; for this reason the capacity of the guard-ring condenser was experimentally compared with the capacity of another condenser without a guard-ring, the capacity of the latter being altered until the capacities of the two condensers were equal. The electromagnetic measure of the capacity of the condenser without a guard-ring was then determined by Maxwell's method. The ratio of the

electrostatic to the electromagnetic measure of the capacity is v^2. The result of the experiments, using Lord Rayleigh's value of the ohm, was that

$$"v" = 2\cdot963 \times 10^{10} \text{ in C.G.S units.}$$

XI. "On the Molecular Weights of the Substituted Ammonias. No. I. Triethylamine." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor, Cambridge, and ALEXANDER SCOTT, M.A., D.Sc. Received June 21, 1883.

The conduct of the experiments relating to a new determination of the atomic weight of manganese recently communicated to the Society* has led us to prosecute some further studies in this field of research. The following note deals with the preliminary results arrived at regarding the molecular weight of a member of a class of bodies which, strange to say, have not been previously selected for accurate determinations of this kind. The substituted ammonias are peculiarly fitted to reveal the effect of small differences from whole numbers in the conjoint values of the atomic weights of carbon and hydrogen. By selecting tertiary amines of high molecular weight it is possible to integrate these small positive or negative increments through the increase in the number of carbon and hydrogen atoms in the substituting radical. There is also a special advantage in employing the fully saturated ammonium derivatives for experiment. Theoretically it ought to be possible to ascertain by this method whether the atomic weight of hydrogen differs from unity, provided the atomic weight of carbon be accepted as sufficiently well defined, from other methods of investigation. The difficulty of getting perfectly pure substances for such work, together with the hygroscopic character of the ammonium compounds, introduces serious difficulties, and for the purpose of testing the accuracy of the proposed method, the preliminary experiments have been made with triethylamine.

The triethylamine employed was made by the action of chloride of ethyl on ammonia, and was transformed into the bromide of tetraethylammonium. This bromide of the fully substituted ammonium was decomposed by dry distillation into triethylamine and bromide of ethyl, and the base separated in the form of the chloride. The free base was separated from the chloride with caustic potash, and after careful drying with anhydrous oxide of potassium was subjected to fractional distillation. The portion boiling between 90° and 91° was converted into the hydrobromate and its equivalent relation to

* "On the Atomic Weight of Manganese," "Proc. Roy. Soc.," vol. 35, p. 44.

silver determined, after the method of Stas, with the following results :—

Weight of salt in vacuo.		Weight of silver in vacuo.		Molecular weight of $(C_2H_5)_3N.HBr$.
6·6248	......	3·9219	......	182·313
8·24088	......	4·8798	......	182·270

A portion of the same fraction of the base was now treated with nitrous acid in order to eliminate traces of primary and secondary amines, and the titration repeated with the following results :—

Weight of salt in vacuo.		Weight of silver in vacuo.		Molecular weight of $(C_2H_5)_3N.HBr$.
5·3165	......	3·1519	......	182·052
4·6237	......	2·74194	......	182·001

The effect of the nitrous acid treatment has been to lower the molecular weight, thus proving the presence of small quantities of bases derived from more complicated radicals than ethyl, probably propylamine.

After these preliminary determinations the whole of the sample of triethylamine was fractionated with great care, and the portion boiling between 90° and 91° C. selected for a repetition of the process. The middle portion of this second distillation boiling between 90°·2 and 90°·4 was again separated into three fractions by a new distillation and the molecular weights of the respective samples of the base determined. The following table gives the results of the different titrations :—

	Weight of salt in vacuo.	Weight of silver in vacuo.	Molecular weight of $(C_2H_5)_3N.HBr$.	Remarks.
I....	7·06272	4·18778	182·025	First samples, boiling point 90—91°.
II....	6·4418	3·8199	182·011	Second fraction of I, boiling point 90°·2—90°·4.
III....	15·46765	9·18495	181·756	First portion of II refractionated.
IV....	11·95685	7·0902	182·012	Middle and chief portion of II refractionated.
V....	13·9522	8·2664	182·166	Highest boiling point, portion of II refractionated.

The molecular weights of the different samples clearly prove that the base is not homogeneous, the presence of bases of higher

and lower molecular weights being revealed by the analysis. No doubt, the amount of this impurity is exceedingly small, but still sufficient to prevent a definite conclusion being reached as to the correct value for pure triethylamine. As Stas's method of titration is capable of giving concordant results within one ten-thousandth of the molecular weight, the large variation, amounting to one four hundred and fortieth of the mean molecular weight of the first and third sample of the last fractionation, shows that the material is by no means pure enough for the problem we desire to solve. At the same time, the middle and largest portion of the last distillation has probably a molecular weight very near that of the pure base, and may provisionally be accepted. If the molecular weight of the hydrobromate is 182·012, then the value for triethylammonium is 102·061; and if we subtract from this the value for ammonium found by a similar method of titration, viz., 18·074 (Stas), the resulting number 83·987 is the molecular weight of the hydrocarbon molecule C_6H_{12}. This value is probably as accurate a value of the molecular weight of a hydrocarbon as has been hitherto determined, and is sufficient to prove that if hydrogen has the atomic weight of unity, then carbon is twelve, and thus the addition of six atoms of carbon to twelve atoms of hydrogen results in a compound the molecular weight of which may be expressed as a whole number, viz., 84, within the limits of experimental error. This value may be due to the summation of positive and negative variations from the respective values of 1 and 12 for hydrogen and carbon required by Prout's law, and therefore in itself would not prove anything about the law of whole numbers in either atom, unless other methods enabled the atomic weight of carbon or hydrogen to be otherwise defined. Now the labours of Dumas and Stas have shown that if oxygen is taken as 16, then carbon is 12·005, so that the number 83·987 for C_6H_{12} would necessitate hydrogen being rather less than 1 instead of being more, as generally acknowledged when $O=16$ is taken as the standard. Whatever conclusions further investigation may induce chemists to adopt, there can be no doubt the present method is capable of very great refinement in the determination of the molecular weights of hydrocarbon radicals, and when exhaustively treated must lead to results of importance. We intend to continue this investigation, employing other bases than triethylamine, and trust to reach more definite conclusions by working on material of greater purity. We will leave for future discussion Schützenberger's investigation on the variability of the atomic weight of carbon, which is very far from being confirmed by the method of verification we have adopted.

XII. "Contributions to the Anatomy of the Hirudinea." By ALFRED GIBBS BOURNE, B.Sc. Lond., University Scholar in Zoology, and Assistant in the Zoological Laboratory, University College, London. Communicated by Dr. M. FOSTER, Sec. R.S. Received June 21, 1883.

(Abstract.)

The author has investigated the following genera:—

RHYNCOBDELLIDÆ.—*Pontobdella, Piscicola, Clepsine, Branchellion.*

GNATHOBDELLIDÆ.—*Aulostoma, Hæmopis, Hirudo, Hæmadipsa, Nephelis, Trocheta.*

The author gives a bibliography of the most important literature upon the group since the appearance of Moquin-Tandon's monograph.

External Characters and Evidences of Segmentation.

The author, in attempting to answer the question—How far in the series of Hirudinean genera do external characters express the metamerically segmented nature of their organisation?—follows in the footsteps of Gratiolet and Vaillant, who have dealt with *Hirudo* and *Pontobdella* respectively in this connexion. The further question—How far do such metameres represent the somites of a bristle-bearing worm?—first suggested itself to the mind of De Quatrefages.

The author shows that these external evidences of metamerism in *Pontobdella* are most complete, and further that they have a precise relation to the metamerism expressed by the internal organisation. The normal somite here presents four annuli of varying size, each with its special and distinct arrangement of papillæ.

The clitellum involves two reduced somites, each consisting of two annuli, the generative pores being placed between these respectively. The nerve-cord exhibits a corresponding condensation in this region. Twenty somites can be readily distinguished, while posteriorly there are indications of several others, which is in accordance with the existence of twenty-three post-oral ganglia and with Leuckart's observations (*Hirudo*) upon the condensation of an even greater number of primitively separate ganglia in this region. They are rudiments of originally existing somites.

In *Branchellion* it may be shown that three annuli comprise the somite, that in the median region, while every annulus bears a branchia (lateral appendage), the most anterior annulus of the somite bears a vascular dilatation at the base of its branchia.

Similar dilatations, although in a more rudimentary condition, exist in a similar position in *Piscicola, Clepsine*, and *Pontobdella.*

In *Hirudo* the external evidences of metameric segmentation are not so pronounced, but minute examination shows that point for point *Hirudo* agrees almost absolutely with *Pontobdella*. The somite comprises five annuli, the clitellum three somites, the generative pores being placed in the two more posterior.

There is an absolute regularity in the position of the nephridal pores, these occurring in the posterior annulus of a somite.

The internal organisation bears the same relation to these external characters which was stated to obtain in *Pontobdella*. The similar characters of other genera are less fully dealt with.

The external characters thus express in the fullest manner the metamerically segmented character of the internal organisation, and these relations are identical throughout the group.

Skin.

The skin consists of—

1. Cuticle.
2. Epidermis.
3. Dermis.

Cuticle.—This presents similar characters throughout the group.

Epidermis.—This consists throughout the group of a single layer of nucleated cells. These vary in size in different genera.

Two varieties of connective tissue may intrude upon the series of epidermic cells:—

1. Pigmented connective tissue cells, and
2. Capillaries of the vascular system.

No pigment is ever developed in the epidermic cells themselves, as is the case in *Peripatus* (Balfour). The extent to which this intrusion takes place (in respect of the pigmented tissue at any rate) varies much in genera and species, and even in individuals. In most leeches it varies also from point to point, producing the coloured pattern upon the surface of the body.

The intrusion of capillaries only takes place in the Gnathobdellidæ; in the Rhyncobdellidæ they stop short of the epidermic series of cells or merely insert themselves between the bases of these cells.

Two modifications of epidermic cells may take place—*

1. They may become glandular.
2. They may become sensory.

1. *Epidermic Glands.*—Two kinds of epidermic glands are to be distinguished.

i. Mucous glands.—These remain dermic in position and occur all over the surface of the body. They may remain small, and in the series of epidermic cells not passing below them (*Piscicola*), or

* The author takes no account here of the origin of the nephridia; they may be, however, glandular modifications of epidermic cells, but if so are much specialised.

becoming larger, they acquire a narrow neck and lie in the dermic layer of the skin.

They attain a larger size among the Rhyncobdellidæ than among the Gnathobdellidæ, becoming immense in *Branchellion*.

ii. Glands which have taken up a "deep" position among or even within the muscular bundles. They present three well marked varieties.

a. Salivary Glands.—These occur in all the genera, in the region of the pharynx, whether that be protrusible or not. In the former case they open directly into its lumen, in the latter their ducts enter into its base and open along its extended lumen. When the pharynx is protruded they are much stretched, when withdrawn, thrown into folds.

β. Clitellar Glands.—These appear to occur in all the genera except Clepsine, in which genus no cocoon is formed for the eggs. They are exceedingly abundant. In the Rhyncobdellidæ, *Piscicola*, *Pontobdella*, and *Branchellion* they occur even far back in the body, and send their ducts forward in bundles to open upon the surface of the clitellum.

γ. Prostomial Glands.—The author has observed this variety in *Hirudo*, *Aulostoma*, *Nephelis*, and *Trocheta*. They form clear contents and send ducts forwards to open upon the prostomial region. They occur all round the mouth, but in great number in the prostomium. The author has not been able to determine their function.

2. *Sensory Cells.*—The author has not dealt with this modification of epidermic cells. Leydig has given full descriptions of these and their derivatives.

Dermis.—This lies between the epidermis and the circular muscles of the body-wall.

It consists of a matrix of connective jelly (for these and other terms with regard to the connective tissues the author is indebted to a paper by Professor Lankester "On the Connective and Vasifactive Tissues of the Medicinal Leech"), in which are to be found the various forms of connective tissue cell described below, numerous and large blood-vessels, and short muscular fibres.

The muscular fibres are not found in *Clepsine*, *Nephelis*, or *Trocheta*.

The lateral appendages in *Branchellion* are dermic developments.

All the connective and vasifactive elements in the dermis, form a packing to the mucous glands of the epidermis.

Muscles.

The author describes the general arrangement of the muscles, recognizing—

Muscles of the body-wall.

Dorso-ventral and radial muscles.

Muscles in the wall of the alimentary canal.

With regard to the pharynx and its muscles, the protrusible pharynx of the Rhyncobdellidæ is to be regarded as representing the whole body in that region, rather than as merely a central region; when protruded it is in fact an anterior portion of the body. The manner in which it protrudes and recedes into a temporary sac would suggest this, but comparison between its structure and the structure of the whole anterior portion of the body in the Gnathobdellidæ shows that such is the case. This can only be made clear by a series of figures which the author gives.

Muscles developed in the walls of blood-vessels.

Muscles developed in connexion with the generative glands.

Muscles in the walls of the vesicle of the nephridium.

Muscles developed in the skin.

Histological Characters of the Muscles.

The muscles are formed of elongated cells arranged either in bundles or lying singly.

These cells may be much branched; such branched cells occur upon the wall of the alimentary tract, and among the dorso-ventral muscles. The cells consist of a cortical and medullary substance, greatly differentiated from one another. The medullary substance is granular and lodges a large oval nucleus in all cases.

Connective and Vasifactive Tissue.

The author has worked out the histology of the connective substance, and traced its various metamorphoses throughout the group.

The matrix consists of a jelly-like substance, which varies much in amount in different genera; its amount determines the "limpness" or rigidity of the leech. *Hæmopis* and *Aulostoma*, whose bodies are always "limp," possess a great quantity, while *Clepsine* and *Nephelis*, whose bodies are rigid, possess very little.

In the matrix are embedded indifferent corpuscles.

The corpuscles undergo certain metamorphoses:—

1. Entoplastic metamorphosis—the cell preserving a rounded form —Vacuolated cells—Fat cells.

A semi-fluid substance accumulates in droplets in the cell, giving it a reticulately vacuolated appearance; such cells resemble Waldeyer's plasma cells. They are very common in *Pontobdella.*

Fat globules also accumulate, and running together form fat cells, similar in character to the fat cells of Vertebrata. This occurs in *Clepsine* and *Piscicola.* This substance formed presents all the characters and reactions of fat.

Rounded connective tissue-cells are rare among the Gnathobdellidæ, *Trocheta* being the only genus which presents such. They occur in masses, and are also very generally arranged in rows, probably prior to their conversion into botryoidal tissue.

2. Ectoplastic metamorphosis—the cells forming fibres.

The cells of most wide-spread occurrence are cells which have elongated, and it may be branched and formed fibres.

It is possible to trace this process; a slightly irregular cell elongates more and more, and its processes become drawn out, so that ultimately little cell-substance is left and a long very fine fibre is produced, the cell all the time doubtless adding to matrix.

In *Pontobdella* these fibres may become elastic. They always run singly.

3. Ect-entoplastic metamorphosis—the cell developes pigment.

a. The cells take no part in the formation of a vascular system.

This series of modifications is well seen in *Pontobdella*. Indifferent cells develope pigment; this may be traced, originating in young animals (recently hatched).

Such cells either remain rounded or they divide into irregular groups, and afterwards become much branched. The process may be shortened, the cell branching, and forming pigment simultaneously.

The rounded cells lie more deeply, the branched cells lie more superficially, and form the pigment of the dermis.

β. The cells take part in the formation of a vascular system—Botryoidal tissue—" Vasofibrous " tissue.

A set of modifications similar to those just described takes place, but intracellular vacuolation taking place at the same time vascular spaces are formed. These come in communication with the capillaries of the true vascular system on the one hand, and with the sinuses on the other.

4. Entoplastic metamorphosis—Vacuolation to form Capillaries.

The capillaries of the true vascular system are probably formed by the vacuolation of indifferent connective tissue cells.

It may be noticed here that in forms where no canalisation of pigmented cells has occurred, the blood is always colourless, while in forms with red blood such canalisation of pigmented tissue has occurred in the formation of the vascular system.

Blood and Blood Spaces.

Blood.—In the Rhyncobdellidæ—

The blood is colourless.

Colourless amœboid corpuscles occur in very large numbers, but present no remarkable histological characters.

In the Gnathobdelidae—

The blood is red, the plasma containing dissolved hæmoglobin.

Colourless amœboid cells certainly occur in large numbers. In *Nephelis* and *Trocheta* these are almost as large as in *Pontobdella*.

In *Hirudo* and *Aulostoma* these, although not so large, undoubtedly exist in large numbers, in addition to free nuclei.

The blood in all the genera coagulates rapidly when withdrawn from the body, filaments of fibrin or some allied substance can be seen forming on the slide.

Blood Spaces.—These belong to two different systems, which are, however, in direct communication, there being only one fluid. The author shows that one system represents the closed vascular system, while the other represents cœlom, vessels, and sinuses.

The vessels may, to a certain extent, be distinguished by their muscular walls, the walls of the sinuses not being muscular.

Communications between these two systems of spaces, vessels and sinuses exist only at certain definite spots, as in the Rhyncobdellidæ; the vascular dilatations at the sides of the body which are most fully developed in *Branchellion* affording means of communication; or else the spaces establishing that communication, although very numerous, have a special mode of formation and a special nature, such spaces constitute botryoidal tissue (Gnathobdellidæ).

The author describes at length the distribution of the vessels and sinuses.

The conclusion at which the author has arrived concerning the cœlom in the Hirudinea may be thus summed up:—

The somewhat scanty embryological evidence which exists upon this point favours the view that the cœlom developes by a splitting in the mesoblast; that it is, in fact, that modification of an enterocœle which Professor Huxley has termed a schizocœle.

This cavity persists to some extent in all the genera, and while it remains most fully developed in the Rhyncobdellidæ, it is reduced to a minimum in *Nephelis* and *Trocheta*, being then represented only by the ventral sinus and its immediate branches.

In the Rhyncobdellidæ, at any rate in *Clepsine*, *Pontobdella*, and *Branchellion*, the cœlomic remnants (sinuses) continue to be lined with cœlomic epithelium cells. In many places they form a continuous layer, but generally some of them have become free and are to be seen floating in the blood. These free cœlomic epithelium cells, which are much larger than the ordinary blood corpuscles, are only to be seen in the sinuses; they are probably too large to pass through the communicating channels. In the Gnathobdellidæ there is no trace of such cells.

A process has been taking place, which the author proposes to term *diacœlosis*—a "scattering of the cœlom"—connective tissue growths having more or less completely filled it up, the remnants forming the sinus system. Different remnants remain in different genera.

The organs which, in animals possessing a well-developed cœlom, lie within that cœlom, either get blocked out by connective tissue growth or remain enclosed in the remnants. The same organs may remain in different remnants in different genera. No better instance can be given of this than the varying position of the nephridial funnel in *Clepsine*, *Pontobdella*, and *Hirudo*.

The lumen of the existing cœlom, as above described, comes into communication with the lumen of a true vascular system, which was *probably either derived at a very early period from the archaic enterocœle, or was formed independently by hollowing in connective tissue cells. That the communication between the two is of a secondary nature, and not a persistence of the original communication, which must have existed if one developed from the other, is indicated by the existence of colourless amœboid cells in the ovarian sac and around the vas deferens in *Hirudo*. These were probably closed at a very early period before the development of hæmoglobin. This may have a phylogenetic bearing only, but it may very possibly be a process which is repeated ontogenetically.

The development of new cœlomic space (botryoidal tissue) may be termed *pseudocœlosis*.

That this new space is "cœlomic" is amply demonstrated by the fact that in its highest development it encloses the nephridial funnel (*Nephelis*), and, further, that such perinephrostomatous portions of it may acquire a definite musculature and the "botryoidal" cells become modified to form a secondary cœlomic epithelium. The interpretation which the author would put upon this process is that an archaic enterocœle gradually undergoes diacœlosis, being replaced by a pseudocœle. This primary and secondary cœlom exist simultaneously side by side in all existing Gnathobdellidæ.

In the Rhyncobdellidæ considerably more of the primary cœlom remains, and the secondary cœlom has not yet appeared upon the scene.

Nephridia.

The nephridia are in all cases tubular organs, opening on the one hand into the cœlom, and on the other to the exterior.

The funnel, the opening to the cœlom, exists in all the genera, although its existence in *Hirudo* and allied genera has always been denied, but it has long been known to exist in *Clepsine*, *Nephelis*, and had also been described in *Pontobdella*.

The condition of these funnels presents a serial modification. In *Clepsine* and *Pontobdella* they are fairly simple, but in *Nephelis* and *Trocheta* they become drawn out into lobes, and in *Hirudo* and its allies this process has been carried to an extreme, the central lumen

has become lost, and the whole has become a many-lobed, ciliated, spongy mass.

Following upon the neck of the funnel is a dilatation into which blood corpuscles are carried by the ciliary current. In *Hirudo* and its allies the ciliated mass above described comes to surround this.

The position of this funnel varies:—

in *Clepsine* it opens into the ventral sinus;

in *Pontobdella* into special perinephrostomatous sinuses;

in *Hirudo* and *Aulostoma* into remnants of a circumtesticular sinus;

in *Nephelis* and *Trocheta* into botryoidal spaces (pseudocœlom).

The portion of the gland which follows upon the funnel exhibits a degenerate condition; this is probably to be accounted for by the fact that the funnel is gradually losing physiological importance, its function in regard to the secretion of nitrogenous waste being taken on by the blood-vessels. Those genera in which the funnel remains best developed exhibit but little capillary development, and *vice versâ*.

Following upon the degenerate portion or testis lobe is a portion with numerous cells containing branched ductules; these collect together and a central duct is formed, which after a long and winding course either opens directly to the exterior (*Clepsine*), or into a space which then opens to the exterior.

The lumen is throughout intracellular in origin.

This description, excepting as regards the funnel, does not apply to *Pontobdella* or *Piscicola*.

The author describes as existing there a most curious network of tubules, apparently continuous throughout the body and not segmented. These tubules are exceedingly small, and their continuity is a very difficult point to determine with certainty.

These tubules are arranged very irregularly, they turn, twist, bend upon themselves, and anastomose with one another in a most elaborate manner. Their walls become very thin at parts, but never present any opening. The walls present a rod-like structure, such as exists in the nephridial cells in other genera. The lumen is intracellular, the cells being much branched and very large.

The author has been unable to trace their connexion with the funnels, or with a series of very rudimentary vesicles, but such a connexion probably exists. The author has also been unable, owing doubtless to the roughness of the skin, to see the external apertures; he has, however, seen them in *Piscicola*, where a similar structure obtains.

The author proposes to reserve any general conclusions as to the systematic position of the *Hirudinea* which may be drawn from these facts for another communication.

XIII. "Reply to a Note by Professor J. E. Reynolds on the Atomic Weight of Glucinum or Beryllium." By T. S. HUMPIDGE, Ph.D., B.Sc. Communicated by Dr. FRANKLAND, F.R.S. Received June 7, 1883.

In the above-mentioned note of Professor Reynolds* the author criticises the results detailed in a paper which I recently had the honour to contribute to the Society,† and draws an inference from the specific heats of different specimens of the metal which I cannot admit to be founded on facts.

Professor Reynolds remarks that all the results obtained by Nilson and myself tend in one direction, viz., to a considerable, though irregular, rise in the specific heat as the impurities diminish.

If, however, we compare the three determinations from which this inference is drawn—

	Percentage of glucinum.		Specific heat at 45°—50°.
Nilson	87·09	......	0·4084
Humpidge.......	93·97	......	0·4453
Nilson	94·41	......	0·4246

we find that, together with a general rise in the specific heat, there is also a fall between that of the second and third sample which is proportionally greater that the general rise. In other words, the irregularity nearly counterbalances the regularity. On the other hand, as I have already stated, my result is probably slightly too high, owing to the heat produced on the absorption of the turpentine by the porous metal. In Nilson's experiments the metal was enclosed in a platinum capsule.

But even admitting this general rise in the specific heat with diminished impurities, it hardly appears that the specific heat of the pure *fused* metal could be as much as 50 per cent. greater than in the crystalline state. It rather appears that glucinum is either an exception to Dulong and Petit's law of atomic heats or to Mendeleeff's periodic law. I hope shortly to contribute some further evidence to the solution of this exceedingly interesting question, and am now making preparations for a revision of the specific heats of the solid elements in the pure state, and at different temperatures.

Next with regard to the purity of the metal as prepared by my process. The 7 decigrammes which were used for the determination of the specific heat was the first sample prepared, and included all I had then extracted. It ought not, therefore, to be compared with

* Read May 24, 1883, "Proc. Roy. Soc.," vol. 35, p. 248.

† Read April 12, 1883, *Ib.*, p. 137.

Nilson's picked 2 decigrammes, but rather with his first sample, containing 13 per-cent. of impurities. Since sending in my paper I have prepared more that 3 grms. of much purer metal, and can now obtain any quantity in yields of 3 to 5 decigrammes for each experiment. The metal is decidedly crystalline in structure, mostly in thin plates of a high metallic lustre, and of a grayish colour resembling iron. No accurate analysis of these samples of the metal has been made, as it has been nearly all used for the attempted preparation of organo-glucinum compounds. But in my next contribution to the Society I hope to be able to give accurate analyses of several samples.

I only briefly alluded to the theoretical aspects of the question in my paper, and do not intend to refer to this until I have more evidence to offer than a determination of the specific heat between 10° and 100° C.; but the very remarkable result arrived at by Professor Hartley from spectroscopic evidence cannot be left unnoticed. This chemist concludes from his experiments that glucinum is a dyad metal, and that its homologues are calcium, strontium, and barium—elements with which it has not the slightest analogy. And it seems strange that Professor Hartley should consider some slight spectroscopic resemblance between glucinum and the metals of the alkaline earths to outbalance all the weighty chemical and physical differences between them. Glucinum differs strikingly from the metals of the alkaline earths, both in the free state and combined as oxide, as hydrate, carbonate, oxalate, chloride, fluoride, sulphate, &c. If glucinum is really a dyad metal (which may *possibly* be the case), its nearest homologues are decidedly magnesium and zinc; not calcium, strontium, and barium.

XIV. "Remarks on Spectrum Photography in Relation to New Methods of Quantitative Chemical Analysis. Part I." By W. N. HARTLEY, F.R.S.E., Professor of Chemistry, Royal College of Science, Dublin. Communicated by Professor STOKES, Sec. R.S. Received June 20, 1883.

[Publication deferred.]

XV. "On a New Standard of Illumination and the Measurement of Light." By W. H. PREECE, F.R.S. Received June 21, 1883.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 15th.

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"Observations on the Colouring-matters of the so-called Bile of Invertebrates, on those of the Bile of Vertebrates, and on some unusual Urine Pigments, &c." By CHARLES A. MACMUNN, B.A., M.D. Communicated by Dr. M. FOSTER, Sec. R.S. Received March 8. Read April 5, 1883.

I. BILE OF INVERTEBRATA.

The liver of Invertebrates is generally considered by biologists to be nothing more than a pancreas in function, but the observations which I have made seem to me to indicate that it discharges other functions in addition to the preparation of a digestive ferment. The colouring-matters found in the bile of Vertebrates do not occur in that of Invertebrates, with one exception, and that is, hæmochromogen in *Astacus fluviatilis* and in *Pulmoniferous Mollusca*, for I have shown in a former paper* that hæmochromogen is present in mammalian bile. The only observations on the colouring-matters of Invertebrate bile with which I am acquainted are those of Sorby,† and some casual observations of Krukenberg,‡ but neither of these observers makes any mention of the facts to which I have to call attention. Hoppe-Seyler§ failed to detect bile pigments and bile acids in Invertebrate bile. With the exception of these three observers, so far as I know, no systematic examination has been made of the bile and extracts of the liver or enteric appendages of Invertebrates.

The animals examined by me were selected from the three sub-kingdoms Mollusca, Arthropoda, and Echinodermata. Among Vermes I also examined *Lumbricus*, *Hirudo*, and *Aphrodite*, with a negative result as regards enterochlorophyll (*Lumbricus* contains lutein in the wall of the intestine). The most striking fact which presents itself in such examinations as I have to record is the wide distribution of one colouring-matter, which is beyond doubt a chlorophyll pigment, in the bile of Mollusca and some Arthropoda, and in the appendages of the

* "Proc. Roy. Soc.," 1880, vol. 31, p. 206.

† "On the Evolution of Hæmoglobin," "Quart. Journ. Mic. Sci.," vol. xvi, pp. 77–85.

‡ "Vergleichend-physiologische Studien an den Küsten der Adria," 1880.

§ "Physiologische Chemie," 1877–1881.

intestine of Echinodermata, *e.g.*, in the radial or pyloric cœca of starfishes. As this colouring-matter is always found in the appendages of the enteron, I propose the name *Enterochlorophyll* for it. *Zoochlorophyll* would be a shorter and more euphonious name, but as chlorophyll has been found in the integument of several Invertebrates, the same name would apply to the latter, hence the former is more suitable.

I shall take the animals *seriatim*, and proceed to describe the results of my examination, prefacing the observations with this remark—that I have relied on the spectroscopic and chemical proofs of the presence of chlorophyll. It is useless to expect that the chlorophyll in the state in which it occurs should be capable of developing oxygen in the presence of sunlight in the livers of Mollusca or in the pyloric cœca of starfishes, &c., so that one is deprived of the aid of the test which is mainly relied upon by recent observers, such as Professor Lankester* and Mr. P. Geddes;† but fortunately the amount of material obtainable allows one to compare the spectra of enterochlorophyll, in different solutions, with those of chlorophyll obtained from leaves, and to study their respective spectra when the colouring-matters are treated by certain reagents. When this is done, the conclusion forces itself on one's attention that the enterochlorophyll, obtained from the sources already mentioned, is the same as that which occurs in plants; for when two colouring-matters have the same spectrum when dissolved in the same medium, and when their respective spectra are altered in the same manner by the same reagent, then we may conclude, according to Vogel and Kundt, that the two colouring-matters are identical. The spectrum of chlorophyll, or rather of the mixture of colouring-matters which compose it, namely, *blue chlorophyll*, *yellow chlorophyll*, and *chlorofucine* is so peculiar ‡ that its presence is easily detected, and its decomposition, or change, by acids gives rise to no less characteristic spectra. I have not determined how much, relatively to each other, of each of these ingredients is present in individual cases, but the differences which do occur are no doubt due to the fact that sometimes one, sometimes the other, is present in greater or less amount, and that other pigments, such as lutein or a xanthophyll, may be present.§ Before describing the colouring-matters obtainable from the bile or liver of Invertebrates, I

* "On the Chlorophyll Corpuscles and Amyloid Deposits of Spongilla and Hydra," "Quart. Journ. Mic. Sci.," vol. xxii, p. 229.

† "Proc. Roy. Soc. Edin.," and "Nature," January 26, 1882.

‡ See Dr. Sorby's paper on "Comparative Vegetable Chromatology," "Proc. Roy. Soc.," 1873, vol. 21, pp. 442–483, and his other papers, a list of some of which is given in last edition (2nd English) of Sachs' "Botany."

§ In those cases in which the band in red is nearer the violet than usual, it is probably due to the chlorophyll being present in the fluid in a more or less acid state. Cf. Russell and Lapraik, "A Spectroscopic Study of Chlorophyll," "Journ. Chem. Soc.," 1882, vol. 41, p. 334.

shall here give the position in wave-lengths of the bands of an alcohol-ether extract of leaves of *Primula* alone, and treated with nitric acid. I have chosen the leaves of that plant since various slugs and snails, whose bile spectra will be referred to further on, feed on them. An alcohol-ether extract gave four bands. I did not measure carefully the bands of the second half of the spectrum, so shall only refer to the bands of the first half:—

1st Band	λ674—643
2nd "	λ622·5—602
3rd "	λ590·5—567
4th "	λ548—530 ?

This solution was filtered, evaporated at a gentle heat, and the residue extracted with rectified spirit and filtered. The solution was a fine green colour and gave a series of five bands as follows:—

1st Band	λ684—634
2nd "	λ618—598
3rd "	λ586—570
4th "	λ546·5—534
5th "	λ484—465

When this was treated with a couple of drops of nitric acid, the green colour became yellowish, and the bands gave the following readings:—

1st Band........	λ661—646	Chart, spectrum 1.
2nd "	λ608—592	
3rd "	λ576—561	
4th "	λ539—521	
5th "	λ502—484?	

In the case of enterochlorophyll the treatment with nitric acid generally makes the solution slightly greenish, although previously it may have been yellow. I believe this is due to the fact that in the livers of Mollnsca, &c., the pigment is present in a more or less reduced condition, probaby due to the action of a ferment on the chlorophyll, or to the fact that *it is sometimes present in the form of a radical or chromogen.**

Mollusca.

Colouring-matters of Liver of Ostræa Edulis.—The alcoholic extract of the liver of *Ostræa* is a greenish-yellow colour by daylight, and more orange-yellow by gaslight, and examined in a deep layer gives a band in red, which is placed over a shading. This peculiarity of a

* Perhaps built up synthetically by the animal itself.

dark band superimposed on a shading is noticeable when a crushed *Hydra viridis* is examined with the microspectroscope, and the same appearance was noticed in the spectrum of a leaf of *Anacharis* with which the *Hydra* was associated. In a thin layer of the alcohol extract of the liver of *Ostræa* another band became detached, covering F. The first spectrum referred to is mapped in sp. 2. The band in red extended from—

$$\lambda 696—684;$$

and the second from—

$$\lambda 509—484.$$

When treated with nitric acid the solution became faintly greenish and then a doubling of the band in red was seen to have taken place, the first band faded very quickly leaving the second, but before it disappeared the following readings were obtained :—

1st Band	....	λ690—674
2nd ,,	....	λ661—651

Finally, the series of bands shown in sp. 3, appeared which measured as follows :—

1st Band	....	λ661—646 or 643
2nd ,,	....	λ608—592
3rd ,,	....	λ576—561
4th ,,	....	λ539—518
5th ,,	....	λ505—484

These measurements show beyond all doubt that *the liver of Ostræa contains a colouring-matter which when treated in alcoholic solution with nitric acid gives the same spectrum as a similar solution of leaf green when treated with that reagent; and therefore the liver of Ostræa contains chlorophyll.* A microscopic examination showed that this colouring-matter as it occurred in the liver was more of an orange-yellow colour than it is generally seen in molluscan livers, excepting *Mytilus* and other Lamellibranchiate Mollusca, which have a marine habitat; *but it is not due to the presence of parasitic algæ.*

Liver of Mytilus edulis.—The liver of this Mollusc contains the same pigment. In the yellow filtered extract two bands were plainly discernible in a deep layer, while in a shallower depth a third became detached at F :—

1st Band	....	λ670—651
2nd ,,	....	λ616—593·5

The latter was placed just before D. With nitric acid the extract became greenish and a series of bands appeared as follows :—

1st Band	λ661—646
2nd "	λ608—589
3rd "	λ576—561 or 558
4th "	λ539—521
5th "	λ505—484

The colouring-matters were seen by the microscope to be deposited in brown granules, and no parasitic algæ could be detected.

In the yellow ether extract of the liver no band in red could be detected, but two feeble bands, one at F, the other between F and G, were visible, probably due to the presence of a lutein pigment. Chloroform extracted no colouring-matter.

*Liver of Cardium edule.**—The yellow alcoholic filtrate of the liver gave a band in red, and a band between green and blue at F. That in red extended from λ678—656, but was difficult to read. With nitric acid the liquid became greenish and gave the same series of bands as before.

Liver of Anodonta cygnea.—When extracted with rectified spirit and filtered a deep yellow solution formed, which showed three bands as follows:—

1st Band.........	λ684—663·5	Sp. 4.
2nd "	λ620—600	
3rd "	λ555—539	

Treated with nitric acid this solution became greenish and five bands now appeared, which were difficult to read, but the first extended from λ678—656, and the fifth probably from λ505 to 484 (?)

In the microspectroscope the bands appeared as shown in sp. 5. In another specimen of *Anodonta* an orange-coloured mass was found in the intestine, which gave two bands like those of reduced hæmatin, and a third close before F, the latter extending from λ505—486.

Liver of Unio.—The appearances coincided with those just given for Anodonta.

All the extracts of the livers of the above Lamellibranchiates were treated with ammonium sulphide with a negative result, and therefore did not contain hæmochromogen.

The colouring-matter of the liver of Anodonta was partially soluble in ether with a yellow colour, and this solution showed practically the same spectrum as the alcohol extract.

Bile and Liver of Cephalopod Mollusca. Octopus.—The only cephalopod which I had an opportunity of examining was *Octopus vulgaris*. The yellow-brown liver became darker on exposure to air, due to the

* Fasting for three days.

oxidation of a chromogen. An alcoholic (rect. spirit) extract of a yellow colour showed four bands and a doubtful fifth; which read—

1st Band........	λ678—661 or 681—658·5	Sp. 6.
2nd "	λ616—600	
3rd "	λ584·5—570?	
4th "	λ552—539 (Fifth uncertain.)	

With nitric acid it became greenish and gave the usual series of bands.

But some slight differences were observable in the behaviour of the colouring matters of this liver towards solvents from those already described. This I think may be due to the greater abundance of the colouring-matters present.* Thus an ether solution gave two bands in red, and was a greenish-yellow colour. The first band extended from λ696—690, the second from λ674—661. The alcoholic solution did not seem to be fluorescent. Chloroform extracted some colouring-matter of a faint yellow colour, giving bands like those of the ether solution, that in red extending from λ684—661. And a bisulphide of carbon solution showed a band in red and one at the blue end of the green. In addition to the two bands mentioned above, the ether solution gave three others (as shown in sp. 7), which measured as follows:—

3rd Band........	λ616—596
4th "	λ570—558 (about)
5th "	λ539—530

and a feeble shading from λ494—474.

When an alcoholic solution was treated with ammonia the spectrum remained unchanged, and caustic soda was similar in its action. Sodium amalgam also failed to produce a change. Sulphide of ammonium developed no bands, and therefore hæmatin was absent. I may here remark that no lutein nor tetronerythrin was found in the liver, nor was any found in the integument or elsewhere, an observation which is important when one compares these results with those obtained in examining the "bile" of Crustaceans and Echinoderms.†

Liver of Buccinum undatum.—The filtrate after digesting the crushed liver in rectified spirit was a golden-yellow colour,‡ and its spectrum was composed of four bands in a deep layer, sp. 8;

* And the appearances suggest that in the liver of *Octopus* this pigment exists in a more highly oxidised state than in other livers, or, perhaps, as *acid* enterochlorophyll.

† I have described the peculiarities of the colouring-matter of the *integument* of *Octopus* in a paper communicated to the Birmingham Philosophical Society.

‡ This had a red fluorescence.

that in red extended from λ674—653·5. Treated with nitric acid it became greenish and the usual series of five bands could be detected. The partially decomposed liver (which had broken down into a greenish-white pulp) in another specimen gave four bands when a strong light was concentrated on its surface, viz., a band in red covering C, another close before D, one just before E, and the fourth between *b* and F. No hæmatin could be detected.

Liver of Fusus antiquus.—The filtered golden-yellow rectified spirit extract showed in a deep layer four bands, and in a thinner a fifth at F. These closely agreed with those seen in the similar extract of liver of *Buccinum;* that in red extended from λ676—660, the next λ620—600, and the third perhaps from λ583—567. With nitric acid the solution became greenish and the five bands already referred to were seen distinctly. No other pigments were detected. With magnesium and acetic acid a negative result was obtained.

Liver of Purpura lapillus.—The golden-yellow rectified spirit extract of the liver gave three bands in a deep layer coincident with the bands of the above alcohol extracts, thus that in red extended from λ678—656. And the colour changed to greenish with nitric acid, the usual series of five bands becoming developed at the same time. Owing to the presence of the purpurogenous glands* in this mollusc, I hoped to find some peculiarity in the pigments of its liver, but it contained apparently only enterochlorophyll.

Liver of Litorina litorea.—The *absolute alcohol* filtrate of the liver was a yellow colour, and gave three bands in a deep layer and a fourth in a shallow depth, which are the same bands seen in other cases. The following measurements apply to the spectrum of the above-mentioned solution:—

1st Band	λ678—661
2nd „	λ620—600
3rd „	λ552—539
4th „	λ516—480?

On treatment with nitric acid the usual series of five bands could be detected with ease.

Liver of Helix aspersa.—When the liver and bile of any Pulmonate Mollusc is examined, except that of *Planorbis*—so far at least as I know—a striking difference between them and those of other Mollusca is apparent, since now for the first time reduced hæmatin is met with both in the bile and in the alcohol extract of the liver, accompanied in most, and probably in all cases, by enterochlorophyll. Dr.

* Schunck, "Notes on the Purple of the Ancients," "Journ. Chem. Soc.," vol. xxxv, p. 589 (1879) and vol. xxxvii, p. 613 (1880).

Sorby* was the first who found reduced hæmatin in the bile of *Helix aspersa*, as well as in that of *Limnæa*, *Zonites*, *Limax* and *Cyclostoma*, he was doubtful as to its occurrence in *Testicella*, *Pupa*, and *Clausilia*. He also found it in *Planorbis corneus* in the liver. I have found it in all Pulmonate molluscan bile which I examined except that of *Planorbis*, and as I shall have to show further on in the bile of the crayfish. In fact its presence seems to be dependent on aërial respiration. Dr. Sorby although he was led to believe that at least two coloured bodies are present, did not notice the chlorophyll pigment which accompanies in most, if not in all cases, the hæmochromogen. The spectrum of hæmochromogen is so peculiar that it is not easily mistaken for anything else, especially after some practice in this line of research. It is interesting that molluscan bile should so closely resemble that of Vertebrates in this particular.

On dissecting fasting specimens of *Helix aspersa* the intestine in the region of the liver is always distended with reddish-brown bile, which invariably gives the reduced hæmatin bands with ammonium sulphide; even before its addition—if the bile be examined sufficiently quickly—they can also be seen. This bile does not give a play of colours with nitric acid, nor does it reduce a solution of cupric hydrate, even after previous treatment (boiling) with acetic acid; it also failed to give Pettenkofer's reaction with sugar and sulphuric acid. In some cases the chlorophyll band in the red can also be detected in the bile itself, but then its position is modified by the nature of the solvent, for instance, in one case, it extended from λ690—666. On treating such bile with an aqueous solution of H_2S, this band persists and two new ones come into view† if not previously visible, the first from λ568·5—555, and the second from λ539—523. Caustic soda narrows these bands, so also does ammonia. An acid, such as acetic, causes their disappearance, while the band in red still persists. Before treatment with ammonium sulphide, when the hæmatin bands are visible, they are nearer the red end of the spectrum; thus in one instance the first band extended from λ576—561. The bile contains a proteid coagulable by heat; this is interesting when we compare it with hæmoglobin. The bands of reduced hæmatin in the bile are not always in the same position, for instance, I found a specimen of *Helix* in February which had been fasting to my knowledge for more than a month. The colour of its bile was almost that of O-hæmoglobin, and without any treatment whatever it showed the bands:—

1st Band	λ570—556·5
2nd „	λ539—526

* "On the Evolution of Hæmoglobin," Quart. Journ. Mic. Sci.," vol. xvi, p. 77.

† The bile at the same time becoming redder. Cf. Sorby's paper, *loc. cit.*, for an explanation of the fact that the position of the bands of hæmatin varies in the "bile" itself without reagents, and after their addition respectively.

Then with ammonium sulphide:—

1st Band........	λ568·5—556·5
2nd „	λ537·5—523

In the alcohol extract of the liver these bands were clearly discernible, and also that of enterochlorophyll. The latter generally was found to extend from λ678—661, and therefore corresponds to the band of enterochlorophyll in other Mollusca, but the colouring-matter is present in smaller quantity than in those examined *supra*, and therefore it is most difficult to measure the bands produced by the action of nitric acid, but they are I believe the same as those already described.

Some specimens of *Helix aspersa* kept fasting for six months still showed in their bile the reduced hæmatin bands, and in one specimen the band in red was still present. I have now specimens in my laboratory which have been fasting nine months, and they appear as vigorous as ever, and a specimen recently examined still contained reduced hæmatin. The drawing of the spectrum of the alcoholic extract of the liver of *Helix pomatia* will represent that of *Helix aspersa*.

Helix pomatia.—The reddish-brown bile from the intestine close to the liver showed the presence of reduced hæmatin with and without sulphide of ammonium, of which (with sulphide) the first extended from λ567—555, and the second from λ536—523. The yellow alcohol extract of two livers showed only the bands of reduced hæmatin with sulphide of ammonium, but in the extract of a third liver the chlorophyll band in red was discernible (see sp. 9), extending from λ678—661. But the last-named pigment is small in amount, as denoted by the action of nitric acid, which, although it caused the hæmatin bands to disappear and left that in red, yet did not *seem* to cause the appearance of any others. The chemical characters of this bile were the same as those in the case of *Helix aspersa*.

Helix citrina.—The yellow alcohol extract of the liver showed the presence of chlorophyll, and resembled those already described in other particulars.

Arion ater: Liver and Bile.—Like all the Pulmonate Mollusca already referred to, the specimens of *Arion ater* examined were kept fasting some days, until they ceased to discharge their intestinal contents, in order to avoid the source of error arising from the fact that the intestine ramifies through the liver, and hence an alcohol extract might contain *intestinal* chlorophyll from the food. The bile is brownish-yellow, and gives generally, without any treatment, the spectrum of reduced hæmatin, which is much more distinct with ammonium sulphide, the bands reading as follows:—

1st Band	....	λ565·5—556·5
2nd ,,	....	λ537·5—528 or 526*

The alcohol extract of the liver showed the presence of reduced hæmatin and enterochlorophyll, the bands of the former disappearing with nitric acid, and that of the latter remaining, being, however, brought slightly nearer the violet, as in all other cases.

In the bile of other specimens the band of enterochlorophyll could generally be seen, but *always* those of reduced hæmatin.

Limax flavus and other Slugs: Bile and Liver.—In the first specimen examined the bile contained reduced hæmatin; but the alcohol extract of the liver did not seem to contain much, if any, hæmatin or enterochlorophyll, but in other cases it contained both colouring-matters. In a specimen of *Limax* of a dark olive colour on dorsal surface, with the margin of the foot tinged with orange, the bile gave the band in red from λ681—656, and, on adding sulphide of ammonium, the bands of reduced hæmatin, the first from λ570—558, the second λ539—526. The alcohol extract of the liver also contained both pigments. In a similar extract of another liver I found that in one part of the liver only enterochlorophyll, in another part only reduced hæmatin was present. Nitric acid changed the colour of the extract to greenish, and *two* of the usual series of bands could be seen. In another specimen of the same colour the greenish-yellow alcohol filtrate of the liver showed four bands, of which the first two belong to chlorophyll, the third to hæmatin.

1st Band	....	λ678—656
2nd ,,	....	λ616—596
3rd ,,	....	λ564—555·5
4th ,,	....	λ543·5—535

This treated with nitric acid gave three of the usual series of bands. (Sp. 10 shows the bile of *Limax* with ammonium sulphide.) Other slugs were also examined with a similar result.

Liver of Planorbis.—The specimens had been kept in clean water, free from vegetable matter, for a week previous to the examination. The livers of five specimens were removed, and, after crushing, digested in rectified spirit; the yellow filtrate gave a band in red from λ678—658·5; and three others, of which that before D belongs to chlorophyll, while the other two were found to be due to traces of hæmoglobin. In a thin layer there was a fourth band at F. In the united alcohol extracts of ten livers no reduced hæmatin could be detected; but that is no matter for surprise, since *Planorbis* contains

* The very slight discrepancies in some of the measurements are no doubt due to the difficulty of determining the measurements of the edges of the bands exactly, and, of course, to the varying quantity of hæmatin present.

abundance of hæmoglobin, and there is no longer need for respiratory hæmochromogen. (It is a remarkable fact that the intestine is not nearly as wide in *Planorbis* as it is in *Helix* and *Limax*. Is this because no intestinal respiration takes place in *Planorbis*?)

Arthropoda.

Among *Arthropoda* I have as yet only examined crustacean bile and liver extracts.

Bile of Homarus vulgaris.—Only one specimen has been examined. The brownish bile of neutral reaction, which got darker on exposure to air, gave a band in red, and an ill-defined band between D and E, placed in the position of that of Moseley's "Actiniochrome."* The former extended from λ681—658·5. The occurrence of the second band is interesting, as the same is seen on examining some of the undissolved pigments in the membrane lining the shell. There was also in an aqueous solution of the bile a third band between green and blue from λ507—484 (sp. 11).

When an extract was made of the liver by means of rectified spirit, the last-mentioned band from λ507—484 was well seen, and there was also a feeble one in red. Sulphide of ammonium hardly affected the spectrum; caustic soda caused the appearance of a band covering D, while that at F seemed to be less distinct; the former was ill-defined, but probably extended from λ600—576. When treated with nitric acid the band at F became better marked, and read λ505—484. Ammonia seemed to cause the disappearance of the same band; and that covering D—which was also produced by caustic soda—again appeared. Magnesium and acetic acid did not affect the band at F, nor did peroxide of hydrogen.

Cancer pagurus: Liver and Bile.—The yellow bile became orange after standing exposed to the air. An aqueous solution gave a shading from λ516—484. No other bands were visible. An absolute alcohol solution of a yellow colour gave a faint band from λ509—484. Treated with nitric acid it became greenish, and a shading from λ509—484 was still visible. Caustic potash did not remove the latter. When the liver was allowed to stand twenty-four hours, and had become a deeper yellow than before, it yielded its colour more readily to absolute alcohol, and this solution gave a band from λ500—480. A chloroformic solution was reddish-yellow, and in this two bands were seen, one from λ509—488, the second λ474—459, the latter fainter than the former; on evaporation of the chloroform an orange-coloured residue was left, which contained oily matter, and could not be purified.

* "Quart. Journ. Mic. Sci.," 1873, p. 143.

An ether solution was pale yellow and showed two bands, the first from λ498—480, the second from λ466—450.

A bisulphide of carbon extract of an orange colour gave two bands, the first from λ530—507, the second from λ496—476 (?) Petroleum also extracted a little colouring-matter, giving a band from λ505—484. Hence the principal colouring-matter present evidently belonged to the class of luteins, of which I believe there are more than one.

Carcinus mœnas: Liver and Bile.—Several specimens of this Crustacean were examined. The bile itself, in most cases, only gave an ill-defined band, between green and blue. The presence of enterochlorophyll was found to be exceptional, and that of lutein or a lutein pigment constant. The colour of the liver in different specimens varies remarkably, thus in some it was yellowish-white, in others, orange, orange-yellow, &c. It became almost black on exposure to the air. In all of the specimens alcohol, ether, chloroform, and bisulphide of carbon extracted the colouring-matter, giving bands in the blue end of the spectrum, which seemed to be due to lutein as already stated, and I believe a similar pigment occurs in the coloured membrane lining the shell, while in some cases tetronerythrin was also present.

In some alcohol solutions two enterochlorophyll bands were visible, the first from λ670—651, the second from λ612—592; with a little nitric acid the colour became slightly greenish. In most cases a band was visible in the same solution from λ505—484.

Ether solutions showed the presence of two well-marked bands, the first from λ498—480, the second λ463—448. A bisulphide solution was orange-red, and gave two bands; the first from λ530—507, the second from λ496—476.

Pagurus bernhardus.—The brown-yellow bile of three specimens gave the band of enterochlorophyll in red, and a shading including *b* and F. The pale-yellow alcohol extract gave the same bands. The solution was too dilute to give a satisfactory result with nitric acid. (Sp. 12.)

Astacus fluviatilis.—While none of the above Crustaceans showed the slightest evidence of the presence of hæmatin in their bile, or in the alcoholic extracts of their livers, the crayfish contains it in considerable amount. So far as I know this has not been discovered before. The brownish-yellow liquid which exudes from the liver gave a faint band between D and E, and a dark one between *b* and F, measuring λ515—488, and on adding sulphide of ammonium two bands appeared which are those of reduced hæmatin. In the bile of some specimens examined the first reduced hæmatin band appeared without any treatment whatever; both were strongly intensified by caustic soda, as was found to be the case in the Pulmonate Mollusca.

It was difficult to get the reading of the second hæmatin band, but the first extended from λ568·5—559·5 ;* the band at F after dilution with water seemed to extend from λ509—484. On exposure to the air the hæmatin bands faded away, but were again brought back with sulphide of ammonium, more distinctly than before. (Sp. 13.) An alcohol extract of the liver showed only some shading at F, and no band could be seen in red, either before or after the addition of nitric acid.

ECHINODERMATA.

In this sub-kingdom I have as yet only examined starfishes and specimens of Echinus.

Uraster rubeus.—The radial, pyloric, or arborescent cœca of starfishes are supposed, I believe, by some to function as a "liver," while others suppose they are of respiratory significance. According to the following observations they do function as the so-called liver of other invertebrate animals, *i.e.*, they—in addition to their probable use in preparing a digestive ferment—serve as organs for the storing, and probably the actual production, of pigments for surface coloration. This at least is the conclusion which spectroscopic study of their colouring-matters suggests.

In different specimens pigments of a different character occur which seem to have a close connexion with those of the integument.

The alcohol extract of the radial cœca of one specimen (the integument of which had a red colour, and which was found to be due mainly to tetronerythrin with probably a small amount of a pigment giving the spectrum and having the negative insoluble characters of Moseley's actiniochrome) was after filtration a fine orange colour, and showed the peculiar absorptive property of tetronerythrin.†

Another specimen had brown radial cœca interspersed with green, the alcohol extract was yellow and gave a band in red and one just before D; the first extended from λ666—639, and therefore was slightly nearer the violet end of the spectrum than in the case of the enterochlorophyll of other invertebrate animals already referred to. The whole of the violet end of the spectrum was shaded up to three-fourths the distance from D to E. The latter shading was cleared up by nitric acid which did not remove the other bands.‡ In a shallow

* The second just before E (see sp. 13), for although it could not be measured by means of the chemical spectroscope it was well seen in the microspectroscope.

† Hoppe-Seyler, "Handbuch Physiol. u. Pathol. Chem. Analyse," 4th ed., p. 220. This pigment is present in *Homarus*, *Cancer*, *Carcinus*, and *Astacus*, as I have found it in the shells of all of them. (Cf. Merejkowski, quoted in "Nature," Jan. 19, 1882.)

‡ Probably owing to presence of tetronerythrin, the colour of which is destroyed by nitric acid.

depth of alcoholic solution a band became detached at F, which on adding caustic soda was slightly narrowed, and read from λ509—490. The ether solution gave similar bands, but in addition it appeared to contain a lutein pigment, as two bands could be seen in the blue and violet, the first from λ498—480, the second from λ466—450. Of course in this and other cases the possibility of the presence of a xanthophyll must not be lost sight of, but in the present stage of the inquiry I believe these bands are due to lutein. A chloroform solution showed the chlorophyll bands like the other solutions, and also two others, of which one extended from λ509—488, the other λ474—456 (?) Another brown specimen of Uraster contained green cœca, and the alcoholic extract of these gave the chlorophyll bands (see 14th sp.), four in number, of which the following are the measurements :—

1st Band	λ674—643	Centre	656
2nd ,, ...	λ614—596	,, ...	604
3rd ,, ...	λ545—534	,, ...	540·5
4th ,, ...	λ516—494	,, ...	505

This solution became greenish-blue with nitric acid and the spectrum of sp. 15 now was seen; the following are the measurements :—

1st Band	λ661—643
2nd ,, ...	λ608—590·5
3rd ,, ...	λ580—561
4th ,, ...	λ539—518
5th ,, ...	λ505—484

Caustic soda changed the spectrum of an alcoholic solution in a remarkable manner, for while the band in red remained as before, that before D became replaced by two narrow bands. They gave the following readings :—

1st Band	λ668—646
2nd ,, ...	λ630—616
3rd ,, ...	λ552—536
4th ,, ...	λ516—494

Only one of the feeble bands referred to above is accounted for, as the other faded away. The solution assumed a yellow colour at the same time.

An alcoholic solution of orange-coloured cœca gave a band in red from λ670—656, and a similar solution of green-brown cœca gave the same band, the latter also formed an orange solution with chloroform which gave two bands, one from λ507—486, the other from λ474—456. In both cases a considerable portion of the blue

end of the spectrum was absorbed, probably from the presence of tetronerythrin.

The greenish-white cœca of another specimen gave the alcohol a yellow colour, which showed a band from λ678—651 (which reading approaches nearer to that of the enterochlorophyll of Mollusca and Crustacea.) When an alcoholic solution of *green* cœca was evaporated on the water bath, a sap-green residue was left, which was only partially soluble in chloroform, a dirty green-brown residue being left still undissolved. The chloroform solution gave four bands:—

1st Band	λ678—637
2nd ,, ...	λ614—596
3rd ,, ...	λ546·5—530
4th ,, ...	λ514—494

Nascent hydrogen, generated by the action of tin and hydrochloric acid, and magnesium and acetic acid, failed to change this colouring-matter, the spectrum produced being the same as that got by the action of nitric acid on alcoholic solutions.*

Asterias aurantiaca.—The brownish cœca when extracted with alcohol yielded an orange-coloured solution which gave the following bands:—

1st Band	λ668—653·5
2nd ,, ...	λ612—596
3rd ,, ...	λ545—534?

On adding nitric acid the usual five-banded spectrum of acid enterochlorophyll appeared. The orange colour of this solution was no doubt due to the presence of tetronerythrin, to which the integument seems to owe its colour.

Echinus esculentus.†—The brown colouring-matter adhering to the *outside* of the intestine and madreporic canal yielded its colouring-matter to alcohol, which became a yellow colour. This showed the band in red and also that before D, and a third feeble one before E. In a specimen of *Echinus* which had greenish-white spines, the perivisceral cavity contained a large quantity of a brownish mass which when dissolved in alcohol formed a brownish-yellow solution, which presented a well-marked enterochlorophyll spectrum with the following measurements:—

1st Band	λ670—653·5
2nd ,, ...	λ612—596
3rd ,, ...	λ545—534
4th ,, ...	λ496—473?

* And oxidising agents gave a similar negative result.

† I am not certain whether the urchin was *Echinus esculentus* or not. The first specimen mentioned above had white spines, the second whitish-green spines; they both belong to the same species, and are commonly known as "Sea-boxes."

Nitric acid turned this extract greenish and the usual five-banded spectrum was seen.

The alcoholic solution fluoresced red. There is reason to suppose that a similar colouring-matter exists on the outside of the shell, which can be extracted with chloroform. There is also another colouring-matter in the perivisceral fluid, which I have described in a paper read before the Birmingham Philosophical Society, and to which I have given the name "*Echinochrome*."*

Summary and Remarks.—It is evident that the livers of Mollusca and Crustaceæ, the pyloric cœca of starfishes, and intestinal appendages of Echinus, all contain in greater or less amount a pigment which is undoubtedly chlorophyll. If this chlorophyll is derived directly from their food, it is strange that it should present similar characters in animals which feed on plants containing different colouring-matters, and that it should still be found in the bile of *Helix* after a six months' fast.

A microscopic examination of a slug's or snail's liver shows the presence of a pigment of a yellowish colour, which within the liver-cells is generally deposited in granules; but there are also bodies which remind one strongly of unicellular algæ, the exact nature of which I have not yet determined. Whether this chlorophyll is built up by the animal itself, or whether it is derived from the food directly, still remains doubtful, but its universal distribution and the large amount present in different livers, teach that it plays an important part in the life of the animal containing it. Although I have failed to change enterochlorophyll into other pigments by oxidising and reducing agents, one cannot help supposing that it does give origin, perhaps under the influence of a ferment, in the animal's body (liver, &c.), to some other colouring-matter. This view being supported by the fact that one can extract from the liver colouring-matters which are present with the enterochlorophyll. Moreover, these colouring-matters are closely related to those of the integument;† for instance, in crabs one finds lutein in the "bile" and lutein in the hypoderm, tetronerythrin in the liver and in the hypoderm and shell, and the same pigment in the pyloric cœca of starfishes and in the integuments of the same starfishes; again, hæmatin in the bile of slugs, and hæmatin derivatives in their integu-

* In that paper several integumental and other colouring-matters are described, which I met with during an examination of the above and other animals. See "Proc. Birm. Philos. Soc.," vol. iii, p. 351, *et seq.*

† In the integument of Crustacea (exoskeleton and hypoderm) I found tetronerythrin, lutein, and a pigment like Moseley's "actiniochrome;" in that of slugs a peculiar hæmatin derivative; in that of starfishes tetronerythrin, a pigment like actiniochrome, and a peculiar hæmatin derivative: these are described in the paper above referred to. "Proc. Birm. Philos. Soc.," vol. iii, 1883.

ment, and so on. Hence the conclusion follows that the integumental colouring-matter is prepared by the liver, or organ answering to the liver. The next question which suggests itself is this: from what constituent in the liver are the pigments built up? I am inclined to think that tetronerythrin is probably built up from a lutein pigment, as they are respectively soluble in the same media, and concentrated solutions of both, of sufficient depths, have the same peculiar absorptive powers for the violet end of the spectrum. Tetronerythrin occurs in abundance, both in the coloured hypoderm and exoskeleton of *Homarus* and *Cancer pagurus*, and lutein is present abundantly in their livers; in *Carcinus mœnas* lutein is present in both situations. Preyer* called attention long ago to the fact that the larva of *Chironomus* contains hæmoglobin, and yet lives on purely vegetable substances; so also does a sheep, a cow, or any other purely herbivorous Vertebrate, and yet hæmoglobin is manufactured in large quantity in their bodies.† It is likely that the radicals which furnish the colouring-matters in the plant would be made use of by the animal, in the synthetical production of such bodies as hæmatin or hæmoglobin for instance, and just as there is a close relationship between lutein and tetronerythrin, so there is certainly a close relationship between lutein and hæmoglobin. For instance, in an egg the only pigment present is lutein, and therefore, if one colouring-matter is derived from another, the hæmoglobin of the chick must be in the first instance derived from the lutein of the yolk. Again hæmoglobin when extravasated into the ovary, gives rise to a pigment presenting a close relationship with that of egg-yolk, so much so that it has been called lutein. For the present I may call attention to the fact that in some cases the simultaneous presence of hæmochromogen and chlorophyll has been demonstrated in the livers of Pulmonate Mollusca, and the amount of one seems to depend on that of the other, for instance, when there was much hæmatin present there was little chlorophyll, and *vice versâ;* this would seem to show that the possibility of the construction of hæmatin from chlorophyll suggests itself, and must not be forgotten.

Bilirubin is an undoubted hæmoglobin derivative (as I shall show further on), and according to Gautier‡ its formula approaches that of chlorophyll very closely; the body chlorophyllan obtained in crystals from an alcoholic solution of chlorophyll gave numbers which led Gautier to the formula $C_{19}H_{22}N_2O_3$, which is not far removed from that of bilirubin, $C_{16}H_{18}N_2O_3$.

* "Die Blutkrystalle," 1871.

† And hæmatin in the bile of some of them, as shown in a former paper. "Proc. Roy. Soc.," 1880, vol. 31, p. 206.

‡ "Comptes Rendus," 1879. "Bot. Zeitg.," 1880. See also Hoppe-Seyler, "Berich. Deut. Chem. Ges.," 1879, and "Bot. Zeitg.," 1879.

The hæmochromogen in the crayfish and in Pulmonate Mollusca is beyond doubt a respiratory pigment, and it seems to me that in addition to the respiration in the branchiæ of the former and in the pulmonary chamber of the latter, this pigment enables the animal to carry on *intestinal respiration*, as in certain crabs and *Cobitis fossilis*. Semper* refers in a note to intestinal respiration in Mollusca, which from the facts above narrated I believe to be carried on in the intestine of Pulmonates by means of hæmochromogen.

Dr. Sorby† refers to the importance of the presence of hæmatin from the standpoint of the evolutionist, so that this question need not be here discussed.

In addition to the classes of chlorophyll containing animals mentioned by Geddes,‡ namely, those which vegetate by their own intrinsic chlorophyll and those which vegetate "by proxy," so to speak, or by means of parasitic algæ, a third class must now be added: those which contain enterochlorophyll in their livers, or other appendages of the enteron.

Conclusions.

(1.) The existence of enterochlorophyll in the liver, or other appendages of the enteron in Invertebrata, is definitely established.

(2.) This pigment occurs in greatest abundance in Mollusca, it occurs less frequently in Arthropoda, and its presence among Vermes is not proved.

(3.) The pyloric cœca of starfishes contain it in abundance, also the intestinal appendage of *Echinus*, which fact shows that the former function like the so-called liver of other Invertebrates.

(4.) The bile of the crayfish and that of Pulmonate Mollusca contains hæmochromogen; in the latter it is generally accompanied by enterochlorophyll, and appears to be concerned more in aërial than aquatic respiration.

(5.) The so-called liver of Invertebrates is a pigment producing and storing organ, as well as being concerned in the preparation of a digestive ferment.

(6.) The presence of hæmochromogen in the bile of Invertebrates is apparently determined by their mode of living; for instance, it is not distributed according to purely morphological considerations.

(7.) It is not impossible that the chlorophyll referred to may be synthetically formed in the animal's body, but any conclusion of this kind is premature at present; although all the facts observed tend to support this view.

* "Animals and their Conditions of Existence," "Internat. Sci. Series," vol. xxxi, note 71 to page 171. Cf. also Darwin's "Origin of Species."

† "Evolution of Hæmoglobin," *loc. cit.*

‡ *Loc. cit.*

II. A few Observations on Vertebrate Bile Pigments.

In spite of the labours of physiological chemists some erroneous ideas still prevail as to the colouring-matters of the bile and their spectra. For instance, one sometimes sees bilirubin described as a green pigment! And in a recently published medical book the author makes the strange statement that human bile owes its colour to biliverdin! Again, the statement is frequently met with that the bile colouring-matters have no absorption spectra. So far is this from being the case, that the spectroscope is capable of detecting the smallest quantity of the biliary pigments in pathological liquids; and I have repeatedly been able to detect bilirubin and biliverdin in pus, urine, and other liquids, by its help.

In former papers* I endeavoured to show how the urinary colouring-matters can be prepared artificially from those of the bile and from hæmatin; and I demonstrated the presence of a colouring-matter indistinguishable from urobilin in mammalian bile. But, like others, I failed to prepare either bilirubin or biliverdin from hæmoglobin or its decomposition products, although the presence of hæmochromogen could be detected in human and sheep bile. I have since been helped in the matter by an experiment which took place under pathological conditions, and which proves beyond doubt that the transformation may yet be accomplished out of the body.

I have also to call attention to the fact that in sheep and ox bile the *principal* colouring-matter *is not* biliverdin; for although I have already shown that the body giving the peculiar series of bands is probably, in fact undoubtedly, a partly hæmatin derivative, I had not studied fully its spectrum nor the best method of procuring it for spectroscopic study. I have also made a few observations on stercobilin and on urobilin, prepared by sodium amalgam from bilirubin, which show that the conclusion is rather premature that the pigment known as febrile urobilin is identical with either, as I had already inferred.

Spectra of Bilirubin, Biliverdin, Bilifuscin, Bilihumin, and Biliprasin.—Bilirubin possesses a peculiar absorptive power for the violet end of the spectrum, and in concentrated solution it absorbs all this end up to D. Dr. Quinlan† proposes that this property should be made use of for the detection of bilirubin in urine. The very abrupt character of the edge of the shading is quite peculiar. It resembles in this respect solutions of tetronerythrin and concentrated solutions of lutein, especially of egg-yolk. The red coloration pro-

* "Proc. Roy. Soc.," 1880, vol. 31, pp. 26, 206.

† "The Application of Spectrum Analysis to the Estimation of Bile in the Renal Secretion of Patients suffering from Jaundice," "Proc. Roy. Irish Acad.," 2nd ser., vol. III (Science). See also "Die Quantitative Spectralanalyse in ihrer Anwendung auf Physiologie, Physik, Chemie und Technologie." By Carl Vierordt. 1876.

duced in urine by boiling with hydrochloric acid has a similar absorptive power for the violet and blue. It is certainly worthy of remark that chlorophyll in plants, and hæmoglobin, tetronerythrin, lutein, and bilirubin in animals, should have this property.

Biliverdin has much the same optical characters as bilirubin, except that more green is transmitted.

The brownish-green alcoholic solution of biliprasin was found to absorb in a deep layer the spectrum up to three-fourths the distance from D to E, but the edge of the shading was not abrupt, but gradually shaded off towards red. It gave the spectrum of Gmelin's reaction when treated with nitric acid; and with caustic soda a peculiar shading or feeble band became detached, placed midway between D and E. The yellow-brown alcoholic solution of bilifuscin gave a similar spectrum, and behaved similarly as regards spectrum with nitric acid and with caustic soda.

Bilihumin when dissolved in alcohol formed a dirty brownish-green solution, which had the same optical properties as the last two colouring-matters. It became yellow-brown with caustic soda, and gave the band already described. It also gave Gmelin's reaction.

Bile Spectra of various Animals.—I have already drawn attention to the peculiar series of bands seen in the bile of certain animals, and shown that they are probably due to the presence of altered hæmatins.* I have examined since then the bile of the tortoise, mud turtle, common ringed snake, and cormorant, but could detect no bands. *During hibernation no urobilin could be detected in the bile of the reptiles.* I have succeeded in detecting this colouring-matter in the alcoholic extract of the liver of *Salamandra maculata* in greater quantity than can be accounted for on the supposition that it was present in the blood-vessels of the liver, *and this indicates that urobilin is probably formed in that organ.* I shall show further on there are reasons for supposing that the hydrobilirubin in the intestine differs somewhat from the urobilins of urine. In the liver of the toad, frog, newt, tortoise, ringed snake, and mud turtle, as well as in that of the pig, cat, guinea-pig, rabbit, and man, I can find no urobilin in the alcohol extract, probably for the same reasons that other chemical compounds which are known to be formed in that organ cannot be detected in its extract, namely, that they are removed as soon as formed.† The alcohol extract of the above-mentioned livers generally showed the lutein spectrum. In the alcohol extract of the liver of a little fish, which I believe was *Blennius pholis*, abundance of tetronerytherin was

* "Spectroscope in Medicine," 1880, and "Proc. Roy. Soc.," 1880, vol. 31, p. 26. It is a very interesting fact that the bile of *Pulmonate Mollusca* and of *Astacus f.* should contain reduced hæmatin like that of certain mammalia in which I have found it, as already communicated to the Royal Society.

† I have found hydrobilirubin in the fæces of the frog and salamander.

found. Now as this pigment occurs in the skin of *Mullus surmuletus* and other fishes, it is not impossible that in some fishes the liver may function in a similar manner to that of Invertebrates, and serve as a pigment-storing organ for surface coloration.

Detection of Bilirubin, &c., in Exudations.—The peculiar series of bands, and the way in which they appear and disappear when solutions of the bile-pigments are treated with nitric acid, serve as accurately for their detection as can be wished. In a former paper I showed the effect of chlorine in traces on such a solution, and the description of the spectrum thereby produced applies to Gmelin's reaction. A band appears before D, then one after D, and one at F; the second one fades away first, followed by that before D, and that at F is left. This finally also disappears. *No other animal colouring-matter behaves in this manner*, hence this test is conclusive. In applying it the colouring-matters should, if possible, be got into solution in alcohol or chloroform, and the nitric acid added to the solution. Care is necessary in the case of the alcoholic solution, and it should be diluted sufficiently with water before adding the acid.

Origin of the Colouring-matters of Bile.—The discrepancies in the statements of physiological chemists as to the identity of hæmatoidin and bilirubin probably arose from the fact that deposits of blood colouring-matter, undergoing all stages of decomposition, have been included under that name. The evidence for the identity is given by Hoppe-Seyler in his "Physiologische Chemie" (pp. 311—313). I have already shown that the same pigments can be formed from hæmoglobin and bilirubin, but an actual proof of the transformation of hæmoglobin into *biliverdin* has not, so far as I know, been brought forward until now.

The liquid* in which the biliverdin was detected had been removed by tapping from the *tunica vaginalis testis* of a case of chronic epididymitis. Six months before the specimen was removed the sac had been transfixed by a needle in the hope of causing absorption, but without effect; three months later it was punctured with a trochar and canula, and about two ounces of blackish bloody serum drawn off, but the fluid again accumulated, and at the end of three months tapping was again performed. The liquid removed on this occasion was a dark bluish-green. Since it was albuminous, a method had to be adopted, before applying the spectroscopic test, which would coagulate the albumen without altering the colouring-matter, and then extract the latter. The liquid showed, however, without any treatment, the presence of a hæmoglobin derivative, which was proved by appropriate tests to be a body intermediate between methæmoglobin and hæmatin; it also absorbed strongly the violet end of the spectrum. The albumen

* I have to thank Dr. Carter, of Birmingham, for this specimen.

was coagulated by absolute alcohol, and thrown on a filter, the mass on the filter exhausted with it, the faintly green filtrate evaporated at a gentle heat. When the residue was extracted with alcohol, and the latter treated with nitric acid after proper dilution, the above reaction and the spectrum of Gmelin's reaction could be made out easily. It is a well-known fact that nitric acid will lead to erroneous conclusions when added to an alcoholic solution of bile-pigments, but the spectroscope always shows whether the colour reaction is due to bile-pigment or not. As well as I could make out, the band before D extended from $\lambda 620$—596, that after D faded quickly away, that at F read from $\lambda 511$—494. By the action of oxidising and reducing agents I found that the pigment present could have been nothing else but biliverdin.

In this case no jaundice was present and the only source from which the biliverdin could have been derived was hæmoglobin, which was first converted into methæmoglobin, then into the pigment intermediate between that and hæmatin, and finally into biliverdin. This transformation was probably one of gradual oxidation, assisted by the action of a ferment, which is doubtless present in hydrocele liquid. That a ferment is present in the testis is probable, as Berthelot has prepared glucose by rubbing up testicular tissue, glycerine, and mannite together. The "hæmatochlorin"* of the placenta of the bitch is probably formed in a similar manner.

Identity of Stercobilin and Hydrobilirubin; Differences from Urobilin of Urine.—The spectra of solutions of hydrobilirubin prepared by the action of sodium amalgam on bilirubin have been already described.† I have again prepared it, and find no differences in the characters of the pigment from those which I described; but I have to call attention to Chart II, sp. 14, of my former paper, which represents an alcoholic solution of hydrobilirubin treated with caustic soda. In that spectrum a band is shown touching C and placed between that line and D, another at D, and a third from *b* to near F. I now find the very same bands in an alcoholic extract of the fæces of the cat, after treatment with caustic soda. They were also seen in extracts of gall-stones, as I have previously stated, *but these bands are not found in an alcoholic solution of urobilin from urine when treated with caustic soda.* Hence if the urobilin of urine is derived from the hydrobilirubin of the intestine, it has undergone some change after leaving the intestine (by absorption).

An alcoholic solution of cat's fæces‡ shows the band at F with great distinctness from $\lambda 505$—484, and after treatment with caustic soda, the orange solution became more yellow and gave a less refrangible band from $\lambda 516$—500 in a shallow layer, but in a deep one that

* Preyer, "Die Blutkrystalle."

† "Proc. Roy. Soc.," 1880, vol. 31, p. 206.

‡ Removed after death from large intestine.

reagent developed a band in red between C and D, and another covering D, exactly as figured in the above-mentioned chart of spectra. The centre of the first was at λ634, of the second at λ589. In an alcoholic solution of artificially prepared hydrobilirubin caustic soda develops the same bands, *but in an alcoholic solution of febrile urobilin they are not seen after it is added, a band in green being seen instead.*

I have isolated febrile urobilin several times, and find its spectra coincide with those previously described,* and that the two bands near D are as much part of the spectrum as that at F. So-called indigo-blue gives a band before D, and "omicholin" one after D, but I find that these bands give totally different readings, and neither was present in the isolated pigment; but this will be again referred to.

The Colouring-matters of Sheep and Ox Bile.—When some brown-green sheep bile in a thickness of 18 millims. was placed before the slit of the spectroscope (one-prism chemical), the spectrum was absorbed at one end at λ744 completely, at the other at λ534; the shading was continued for some slight distance over the visible spectrum; between these points three bands were visible, viz.:—

1st Band	λ666—634
2nd „	λ610—587·5
3rd „	λ576—561

In the same depth of brown ox bile three bands are also visible which had the same position.† Absolute alcohol was now added to both specimens of bile, and they were then filtered.

The ox bile filtrate appeared to have a slightly red fluorescence; it had a red colour (by gaslight it was dichroic, *vide infra*), and gave four bands as follows:—

1st Band	λ656—643.	Centre	648·5
2nd „	λ612—584·5.	„	596
3rd „	λ578—564.	„	570
4th „	λ536—518.	„	528

When this was shaken with chloroform, the latter took up the colouring-matter and was dichroic; looked down upon in a beaker, it appeared red, but had a greenish tinge with transmitted daylight. This gave a spectrum of four bands:—

1st Band	λ651—634
2nd „	λ604—581·5
3rd „	λ571·5—561
4th „	λ534—516

* "Proc. Roy. Soc.," 1880, vol. 31, p. 26.

† Maps of the spectra of ox and sheep bile are given in the paper published in "Proc. Roy. Soc." already referred to.

This solution looked greenish on a white dish, and after evaporation over the water-bath left a residue of a peculiar colour, a kind of mixture of green, brown, and red. It was soluble in alcohol, giving the spectrum already described (and the dichroism); but a portion remained undissolved, which had a lavender colour and dissolved in chloroform, forming a rose-red solution (gaslight, green by daylight), giving the same spectrum as the former chloroformic solution. Both nitric and hydrochloric acid developed bands in this chloroformic solution, somewhat like those of sulphate of cruentine or acid hæmatoporphyrin, but having different measurements and peculiarities of shading. The colouring-matter of ox bile is also soluble in ether, and this gives the same series of bands as an alcohol or chloroform solution.

A chloroformic solution of sheep bile showed in addition the band of urobilin from $\lambda 502$—482.

Several experiments were made on these colouring-matters, but as my object is merely to show that: (1) chlorophyll as such cannot be present in sheep bile, since the band in red is in an entirely different position; and (2) to compare the spectrum of sheep and ox bile with that of Gmelin's reaction, I will not here give further details, especially as I hope to study the relationship of these colouring-matters to those of various decomposition products of hæmoglobin and chlorophyll at some future time.

If chlorophyll were present, not only would the band mentioned above have been nearer the red, but the spectrum of acid chlorophyll should have been got with nitric acid, which was not the case. With regard to the spectrum of ox bile, Hoppe-Seyler* makes a statement which cannot now be accepted. After describing the spectrum of ox bile, he says that the same body can be got by the oxidation of bilirubin, biliverdin, or bilifuscin, with nitric acid or bromine-water, when these are made to act on chloroformic solutions. According to my observations, a body of totally different spectrum appearances is produced under these circumstances. Thus, if the *bilicyanin* of Heynsius and Campbell† (which according to Hoppe is identical with the sheep bile pigment, and which gives the bands before and after D) be prepared according to the following method, which I find answers for the purpose, a different conclusion is arrived at. An aqueous alkaline solution of bilirubin was treated with nitric acid until the black band was seen at F; it was then quickly poured into a separating funnel, diluted with water and shaken with chloro-

* "Handbuch," *loc. cit.*, p. 211. It cannot be denied that the pigment of sheep and ox bile is partly a hæmatin derivative, and it does present some *likeness* to chlorophyll. It may possibly be a mixture of derivatives of both. I hope to investigate this shortly.

† "Arch. f. d. Ges. Physiol.," IV, p. 497.

form, but the chloroform failed to take much colouring-matter up; a deposit formed, being precipitated by the chloroform, which, when dissolved in alcohol, formed a fine blue solution, giving a band before D and one at F, the first from λ616—590·5, the second λ507—486.* By further addition of nitric acid to this solution it could be made to show a band after D; it was then shaken with chloroform, and the latter separated off was a purple-red by gaslight, and gave

1st Band.........	λ620—592 or 590·5
2nd „	λ583—548
3rd „	λ509—486

If these measurements are compared with those of the bands of a similar solution of ox or sheep bile colouring-matter a wide difference is apparent. Hence the pigments of sheep or ox bile and that produced from bilirubin by oxidising agents are not the same.

Another spectrum can be got by acting on bilirubin in alkaline aqueous solution with potassium permanganate; by cautious addition of this reagent a band appears in the space between D and E, and touching the latter line; but on adding more permanganate it disappears and a green solution is formed, which is merely due to the action of the caustic soda, used in dissolving the bilirubin, on the permanganate.

III. On some Unusual Urine Pigments, &c.

In former papers I have described four pigments, namely, *febrile urobilin*, *normal urobilin*, *urohæmatin*, and *urolutein*. I have here to refer to the spectroscopic detection of *indican* in urine, to *uroerythrin*, and to a peculiar colouring-matter met with in pale urine. I am more convinced than ever that only by the use of the spectroscope can these pigments be distinguished from each other; and the condition of the first three furnishes one with an exact test of the amount of the metabolism of hæmoglobin, and of the oxidation or reduction going on in the organism. I have first to refer to some matters bearing on former results.

The Spectrum of Febrile Urobilin.—I have made measurements of the bands of solutions of this pigment, as those in my first paper† were not made with the help of the more exact method of measuring now at my disposal.‡ The pigment was obtained by means of the method described in former papers.

* A *shading* was perceptible after D from λ589—548?

† "Proc. Roy. Soc.," 1880, vol. 31, p. 26.

‡ All the wave-lengths in this paper are calculated by means of an interpolation curve adapted to the scale of a one-prism chemical spectroscope, as the microspectroscope cannot be sufficiently relied upon for accurate work.

The chloroform solution gave three bands:—

1st Band	....	λ612—592	Centre	....	604
2nd "	....	λ576—552	"	....	564
3rd "	....	λ511—488	"	....	499

The ethereal solution also gave three bands:—

1st Band	....	λ610—589	Centre	....	600
2nd "	....	λ576—545	"	....	562
3rd "	....	λ509—488	"	....	498

An absolute alcohol solution gave:—

1st Band	....	λ610—587·5	Centre	....	600
2nd "	....	λ576—558	"	....	567 ?
3rd "	....	λ516—488	"	....	502

This with caustic soda gave two bands: the first from λ583—561, and second, λ521—507.

In a deep layer of an aqueous solution a faint shadow could be seen covering D; by treatment of this aqueous solution with permanganate of potassium, this shadow could be made to disappear; that at F was narrowed, became hazy, and finally disappeared. So that just as normal urobilin can be reduced back to febrile urobilin, as I formerly showed, so can febrile be oxidised into normal urobilin.

By acting on an alcoholic solution with caustic soda no bands at C *and* D *in the position of those obtained in a similar solution of hydrobilirubin artificially prepared, or in a similar solution of stercobilin, could be seen.* Hence febrile urobilin is not identical, as previously stated (*supra*), with hydrobilirubin or with stercobilin. If it is derived from stercobilin, it has undergone some change before its excretion in the urine.

Dr. Harley's Urohæmatin.—I have repeated Dr. Harley's experiments on "urohæmatin"* and find not only is his pigment totally different from any that I have described, but a mixture of several decomposition products of chromogens, produced by the action of heat and mineral acids on them; nor by following out his directions could anything approaching a pure residue be obtained. Hoppe-Seyler's conclusion that his results are only of historical value may therefore be accepted. I mention this, as I had omitted in former papers to say that Dr. Harley had inferred, long before the spectroscope was used in such investigations, that the colouring-matter of urine was derived from that of blood, but the resemblance between his "urohæmatin" and the pigments which I have described lies only in the name.

* "Ueber Urohæmatin und seine Verbindung mit Animalschem Harze." "Aus den Verhand. der Physicalisch-Med. Gesellsch. zu Würzburg," Bd. 5, 1854. Also "Pharmac. Journal and Trans," vol. xii, 1852–53, p. 248.

Urohæmatin and Hæmatoporphyrin.—Since I described urohæmatin I have met with it five times, four times in the urine of acute rheumatism and once in the urine of a case of so-called "idiopathic" pericarditis. Its occurrence in the urine in the former disease appears to be pretty constant, and this is interesting when its artificial production is borne in mind, since it is produced by the action of nascent hydrogen on acid hæmatin. It would seem to indicate that an acid fermentation, attended by the destruction of considerable quantities of hæmoglobin, is present in that disease, and this agrees with the well-known fact that a product of such fermentation is present in the blood of acute rheumatism, namely, lactic acid.* Since this acid is produced from glycogen (as well as in the muscles), and since hæmoglobin is broken down in the spleen and sent into the liver (being helped there by the rhythmical contraction of the spleen, which Roy has discovered), to be changed into the colouring-matters of bile and urine, it would appear that the liver is the principal seat of this peculiar fermentation. The presence of urohæmatin in the urine at all events indicates that the normal oxidation of hæmoglobin down into urobilin is not taking place, and that probably the bodies with which urohæmatin is associated are also insufficiently oxidised. Hence its occurrence is of great pathological importance. I find that when present in sufficient quantity it can be detected without isolation, as its bands are easily seen in the urine itself. In this liquid it can be made to show two spectra which are equally characteristic, namely, that of *acid* and *neutral urohæmatin*, and these can be distinguished from those of methæmoglobin or of hæmatin, by the use of sulphide of ammonium, as this has no effect on urohæmatin.

It occurs in the urine in the condition of neutral or slightly alkaline urohæmatin, and this shows four bands—a feeble one between C and D, two between D and E, and a third close to F (see sp. 16); the second and third bands might easily be mistaken for acid methæmoglobin, but sulphide of ammonium, as stated above, serves to distinguish them. Moreover, when it is present, Day's test fails to give a blue coloration. The bands just referred to gave in the urine the following measurements:—

1st Band	λ620—608
2nd "	λ580—561
3rd "	λ542—530
4th "	λ507—486

* The presence of lactic acid in the blood of patients suffering from acute rheumatism is inferred from the experiments of Dr. Richardson. There is at all events an excessive production of acid of some kind in that disease. I failed to produce any endo- or peri-carditis in guinea-pigs and rabbits by the injection of lactic acid into their peritoneal cavities.

On adding an acid, such as nitric, in small quantity, two very characteristic bands came into view:—

	....	λ594—587·5 (sp. 17)
2nd ,,	....	λ556·5—542

At first sight they seem to resemble the bands of acid hæmatoporphyrin, but they are different. To prove this I adapted an interpolation curve to the drawing of the spectrum of that body figured by Hoppe-Seyler and found the following measurements for acid hæmatoporphyrin:—

1st Band	....	λ601—589
2nd ,,	....	λ566—546

For his alkaline hæmatoporphyrin the following measurements were found:—

1st Band	....	λ630—615
2nd ,,	....	λ601—565
3rd ,,	....	λ551—539
4th ,,	....	λ521—498

Hence they are not identical.

I have also prepared hæmatoporphyrin in different ways, and find that urohæmatin, although closely resembling it, is not the same body, nor is it identical with Baumstark's* *urorubrohæmatin* or *urofuscohæmatin*, as the solubility of these bodies is different. The other characters of the isolated pigment are described in my last paper.†

Spectroscopic Detection of Indican in Urine.—In cases where Jaffe's test fails to detect indican in urine the spectroscope is able to detect it.‡ The best method, and one which, after repeated trials, I find easily applied, is somewhat like that of Stockvis, but while he used nitric I use hydrochloric acid. The urine is mixed with about its own bulk of hydrochloric acid and boiled; after cooling, it is agitated with chloroform, not violently shaken, and left to rest for some time. When the urine contains indican the chloroform-layer is more or less violet and shows a band before D. The latter corresponds in position with that of the so-called "omicholin." In applying the test the urine always gets more or less red, and there is a strong absorption of the violet end of the spectrum; now I find that in cases where *no indican* is present this reddening takes place. Some authorities say that this reddening is due to urrhodin, others that it is due to indigo-red, while others maintain that these two colouring-matters are the same. But

* "Berichte der Deutsch. Chem. Gesellsch.," 1874, Bd. 7, p. 1170.

† "Proc. Roy. Soc.," 1880, vol. 31, p. 206.

‡ Jaffe's test is described in most works on the urine. See *e.g.*, Neubauer and Vogel, "Analysis of Urine," American translation, 1879.

neither view is quite correct. The reddening produced by boiling with hydrochloric acid in *normal* urine is certainly not due to the oxidation of indican, since I can detect no indigo-blue under these circumstances, but it is partly due to the oxidation of the chromogen of urobilin, since the latter goes into the chloroform when the mixture is shaken with it, and can be detected by means of the spectroscope. At the same time the oxidation of the urobilin chromogen does not account for all the red coloration produced in normal urine and for the presence of the colouring matter, which then possesses an absorptive power for the violet end of the spectrum. Hence the so-called "indigo-red" in urine is not due to the presence of indican, as it is present in quite normal urine, while indigo-blue cannot be obtained from the same urine in all cases. I have, therefore, to agree with those who maintain that the above-described absorption of the violet end of the spectrum is due to "urrhodin," and who also maintain that urrhodin and indigo-red are different colouring-matters.

In most cases where I have detected indican by this method I have noticed a band in the urine which seems to be connected in some way with its presence. Thus, in the urine of a case of aphasia, due to embolism from mitral disease, the addition of hydrochloric acid to the urine (which was a reddish colour) made it redder in colour, and a band became developed, beginning about half way between D and E, and extending nearly to E. It probably extended from $\lambda 558$—534. The urobilin band was also visible. On boiling the urine with hydrochloric acid, cooling, and agitating with chloroform, the latter became a faint purple-red colour; this showed two bands with their centres at $\lambda 608$ and $\lambda 571 \cdot 5$. On evaporating the chloroform, alcohol took up the pigment from the residue, and gave two similar bands. In this case the band before D was less shaded than that after D, but in other cases the reverse was the case; this teaches that not one, but *two distinct colouring-matters* are indicated by the presence of these bands, for if only one colouring-matter was indicated by them then they should possess the same *relative degree of shading* when dissolved in the same medium *in different cases*. Another specimen of urine gave* the band referred to above, namely, extending from half-way between D and E to near E, and on treating, as before, with hydrochloric acid and chloroform, the indigo-blue band before D was well seen, the band after D uncertain. The urobilin band was also visible. In a specimen of urine (from a case of enteritis), which was a kind of olive-brown colour, the same treatment developed the bands, of which one extended from $\lambda 620$—592, the other $\lambda 583$—561 (in a chloroformic solution). Possibly a bile pigment partially oxidised might be mistaken for the pigment, but in

* Without any treatment whatever.

the latter case the addition of nitric acid would cause the disappearance of the bands near D, while in the case of indigo-blue no effect would be produced. I have tried the effect of the above treatment on solutions of bile-pigment and on bilicyanin, and find that they behave quite differently from indican. When febrile urobilin, or urohæmatin, or uroerythrin, or other pigment is present, this test is still applicable, and will probably be found of great use in clinical work. (See sp. 18.)

Uroerythrin.—A hot alcohol extract of uroerythrin from pink urates always gives a double absorption band from about three-fourths the distance between D and E to beyond F, and therefore differs most decidedly in spectrum from that figured by Dr. Thudichum;* instead of showing the three well-defined bands figured by him, it always gives a hazy double band or shading in the position described. The uroerythrin which I examined always answered to the description, as regards chemical character, given in books on physiological chemistry, which I need not repeat. I have figured this spectrum in sp. 19, for comparison. I cannot trace any connexion between uroerythrin and indican or urobilin, but it often accompanies urohæmatin; and it will be found probably derived from a radical belonging to the aromatic group of carbon compounds.

A peculiar Red Colouring-matter in Pale Urine.—When urine becomes red on the addition of a mineral acid one is apt to conclude that this is due to the formation of indigo-red or urrhodin, or possibly that oxidation of the chromogen of urobilin has taken place, or that a bile pigment may be present, but the specimens of urine which I am about to refer to, contained a pigment which was not one nor the other, as its various spectra showed.

The specimen of urine was sent to me by Dr. Carter of Birmingham† who informed me that it came from "an anæmic sickly fellow" who was taking copaiba and sandal-wood oil for chronic blenorrhœa. I may here state that the urine of other patients taking these drugs failed to show any sign of the presence of the pigment. The first specimen of urine only amounted to about an ounce, and owing to an accident half of it was lost. It had a pale straw colour, was faintly acid, and gave the band of normal urobilin.

When treated with nitric acid in the cold it changed to a splendid ruby-red colour and gave a very feeble band at D, and a very dark one between D and E and touching E, the latter from $\lambda 558-534$; the feeble shading perhaps from $\lambda 612-589$?; but on boiling with nitric acid this red colour was destroyed, it became yellow, and the bands were no longer seen. On adding hydrochloric acid the same feeble band appeared at D, and in a deep layer the whole spectrum up to

* "Journ. Chem. Soc.," 1875 (2), vol. 18, p. 389.

† On January 24, 1882.

near D was absorbed, in a thinner layer three other bands were visible, the colour of the solution changing to a fine carmine-red. The three bands read as follows:—

1st Band	λ558—534
2nd „	λ516—496
3rd „	λ476—462 (see sp. 20 and 21)

On boiling this solution the colour was not destroyed, but the band in violet disappeared. When agitated with chloroform or ether no pigment went into solution. When the red liquid got by adding hydrochloric acid to the urine was treated with caustic potash in excess the colour disappeared, but again reappeared on adding hydrochloric acid in excess.

But in the second specimen obtained three weeks after the above examination, from the same patient, the chemical and spectroscopic characters differed slightly from those of the first specimen, but were essentially the same. On adding about one quarter of its own volume of nitric acid the liquid assumed a splendid red colour, and when agitated with chloroform, the latter removed the colouring-matter forming a lake-red solution; by means of daylight the spectrum was seen to consist of one broad band across the middle of the spectrum, from near D to half way between F and G, and on dilution this band could not be divided into two. After evaporation of this solution a pale yellow residue was left, the change of colour being due probably to the action of the heat, and a little of the acid left in the chloroform, on the pigment. If too much nitric acid was added it also lost its red colour. With nitric acid alone, a band could be seen between D and E and touching the latter line.

On treating the urine with hydrochloric acid and shaking with chloroform, the latter assumed a fine carmine colour, and after filtering gave the same kind of absorption as the nitric acid treated liquid, the broad band extending from between D and E to beyond F. Ammonia at once discharged the red colour of this solution, but it reappeared on acidifying afresh; caustic potash and caustic soda acted similarly. On adding alcohol to a chloroformic solution the colour changed to orange, and then only the violet end of the spectrum was absorbed.

The red colour produced by nitric acid, or hydrochloric acid, was also taken up by agitating with bisulphide of carbon, and this gave the same kind of spectrum as the chloroform solution.

It was interesting to find out whether indican was here present; the urine was therefore boiled with hydrochloric acid, and after cooling shaken with chloroform, it then gave the indigo-blue band before D, and another band after D; and on evaporation left a bluish-black residue. A portion of the latter was soluble in alcohol, forming

a purple solution which gave two bands: 1st, from λ630—589; 2nd, about λ573—555. Hence this urine did contain indican.

Although the spectroscopic characters of the second specimen did somewhat resemble those of urrhodin, yet those of the first were quite different, and hence I have concluded that the colouring-matter was not urrhodin. It also differs from the pigment described by Plosz,* and seems from the description of "Uroroseïn" of Nencki and Sieber† to resemble that pigment, which showed in amylic alcohol a band between D and E with its maximum of shading at λ557. It is at all events closely related to urrhodin. Unfortunately I had not material enough for further study.

In the urine of progressive pernicious anæmia a peculiar colouring-matter was noticed, which after boiling the urine with hydrochloric acid and shaking with chloroform, imparted to the latter extract a fine red colour, giving a band between D and E, but I hope to study the pigments of the urine of that disease more fully at some future time.

EXPLANATION OF SPECTRUM CHART.

Sp. 1. Alcohol extract of chlorophyll of *Primula* (obtained as described in the paper) treated with nitric acid.

" 2. Alcohol extract of liver of *Ostræa edulis*, showing band of enterochlorophyll in red at B.

" 3. The same solution with nitric acid, which developes exactly the same spectrum as 1.

" 4. Alcohol extract of liver of *Anodonta cygnea*. The greater breadth of band at B is due to fact that more chlorophyll is present than in the liver extract of which 2 is the spectrum.

" 5. The same solution with nitric acid. The slight difference of position of the bands in this and spectra 1 and 3 is due probably to a little more, or a little less, acid having been added.

" 6. Alcohol extract of liver of *Octopus vulgaris*. This appears to give ***without any treatment*** a spectrum closely resembling 1, 3, and 5.

" 7. Ether extract of the same liver, showing double band in red. In some cases this double band, or another like it, is got by treating enterochlorophyll with nitric acid.

" 8. Alcohol extract of liver of *Buccinum undatum*. This, like the extract of liver of *Octopus*, appears to contain acid enterochlorophyll; *cf.* it with 6, 5, 3, 1.

" 9. Alcohol extract of liver of *Helix pomatia*, showing band of enterochlorophyll and those of hæmochromogen (reduced hæmatin).

" 10. Bile of *Limax flavus*, treated with a little ammonium sulphide, showing the bands of reduced hæmatin. The slight difference in the position of the latter and those of 9 is due to the solvent, alcohol in case of 9 and "bile" in case of 10.

" 11. Bile of *Homarus vulgaris*, showing presence of a pigment identical with one

* "Zeitschrift für Physiol. Chemie," Band VI, Heft 6, 1882.

† "Journ. Chemie," xxvi, 333–336.


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which occurs in the coloured membrane lining the shell, and probably identical with Moseley's "actinochrome," also traces of enterochlorophyll, &c. (From three different depths.)

Sp. 12. Bile of *Pagurus bernhardus.*

" 13. Bile of *Astacus fluviatilis*, showing presence of reduced hæmatin (sulphide of ammonium having been added). This spectrum is a combination of the results of examining a deep and shallow layer of "bile."

" 14. Alcohol extract of cœca of *Uraster*, showing presence of enterochlorophyll.

" 15. The same with nitric acid. Cf. 5, 3, 1, &c.

" 16. Spectrum of urine containing urohæmatin (without any treatment whatever). This is due to the presence of *neutral* urohæmatin.

" 17. Spectrum of urine containing urohæmatin treated with a couple of drops of nitric acid; this is due to presence of *acid* urohæmatin.

" 18. Appearance of a chloroform solution of indigo-blue and, perhaps, omicholin got by boiling urine with hydrochloric acid, cooling, and shaking with chloroform. The band at F is due to febrile urobilin.

" 19. Alcohol extract of uroerythrin from pink urates (got by boiling with alcohol, &c.).

" 20. Urine containing a peculiar red colouring-matter mentioned in the paper. This spectrum was got by treating it with nitric acid.

" 21. The same urine with hydrochloric acid. This spectrum is a combination of three, as the feeble band near D is not seen in a depth which shows the other two.

"Experimental Determinations of Magnetic Susceptibility and of Maximum Magnetisation in Absolute Measure." By R. SHIDA, Thomson Experimental Scholar, University, Glasgow. Communicated by Sir W. THOMSON, F.R.S. Received October 10, 1882. Read November 23, 1882.

[PLATES 9—16.]

The fact that there exists a limit to the magnetisation of a soft iron bar was first demonstrated by Joule, who, in 1840,* made a number of experiments on the sustaining power of an electro-magnet, and showed that when the current in the exciting coil is made stronger and stronger, that power tends to a certain definite value, or in other words, the magnetisation of the iron core attains a maximum.

In 1861, an interesting research on the magnetic properties of iron was made by Thalén, who determined, among other things, the magnetic susceptibility† of different specimens of soft iron in absolute measure for the first time. The units of length, mass, and time employed by Thalén were respectively a millimetre, a milligramme, and a second.

Joule and Thalén were followed by several, most of whom, however, made experiments without giving the results in absolute units; but amongst the few who have not overlooked the importance of such a system of units, Rowland made by far the most important investigations upon the subject. He determined not only the magnetic permeability or susceptibility of certain so-called magnetic bodies, but also the maximum magnetisation of those bodies in absolute units, using the metre, the gramme, and the second as the units of length, mass, and time.

The method of Thalén and that of Rowland are essentially the same, inasmuch as they depend upon the same electrodynamic principle, that an electric current induced in a closed circuit due to sudden creation or disappearance of magnetic lines of force, is proportional to the number of lines of force thus introduced or withdrawn, cutting the circuit. But one notable difference of the two methods lies in the fact that the one used ellipsoids or cylindrical rods of great length, while the other chiefly used rings or endless rods to experiment upon. The chief advantage of an electromagnetic method such as the above, is, as has been remarked by Sir William Thomson in his paper on the "Electrodynamic Qualities of Metals, Part VI,"‡ the ease and rapidity

* Joule's Collected Papers, page 34, from "Sturgeon's Annals," vol. v, page 187.

† Sir William Thomson, "Papers on Electricity and Magnetism," p. 472.

‡ "Phil. Trans.," 1876, p. 693.

with which the results can be obtained; while its disadvantage is revealed in the fact that it does not show either slow changes of magnetisation or the distribution of magnetism.

The following results of the experiments which have been made at the Physical Laboratory of the University of Glasgow, are given in absolute measure in which a centimetre, a gramme, and a second are taken as the units of length, mass, and time respectively, and were arrived at by means of the direct magnetometric method given to me by Sir William Thomson (who described and explained the method at the recent meeting of the British Association at Southampton in the Section A), as founded upon a method originated by Coulomb and discussed mathematically by Green. This method possesses some important advantages over the electromagnetic method; for instance, it shows at any moment any change of magnetisation of the body experimented on (which is of great practical utility in investigations of this kind); it affords an excellent means of illustrating the distribution of magnetism in the body, and it enables us to experiment upon a long thin bar, subjecting it to different strengths of magnetising forces, and to various amounts of longitudinal stress, and at the same time to determine in absolute measure, the magnetisation and magnetic susceptibility of the bar under these varied circumstances, which is an original feature of the investigations I am going to describe. These advantages, however, do not exist without disadvantages. That the execution of careful investigations involves a considerable amount of time, is a serious disadvantage of this method. After some preliminary studies, the orderly experiments were commenced about the middle of February last, and have since been carried on from day to day without intermission up to the end of May.

A number of thin wires and of thick bars of iron and steel were experimented upon. The accompanying sketch (Plate 9) shows the arrangement of the apparatus employed in experimenting on thin wires. A reflecting magnetometer, M, which consists of a mirror carrying at its back three small magnets and suspended by a single silk fibre about 5 centims. long, was placed on a convenient stand nearly 2 metres above the floor of the laboratory. S is a white paper screen divided into half millimetres, and bent into a circular arc of a metre radius. It is fixed at a distance of exactly 1 metre from the magnetometer, and was used to observe the deflections of the magnetometer needle, which were read by the image of a fine wire fixed vertically in front of a paraffin lamp, L, secured just behind the scale as in a Thomson reflecting galvanometer. N is a magnet of semicircular shape meant to control the strength of the field at the point where the magnetometer needle is suspended. It was mounted on a suitable stem in front of the magnetometer needle, with its length in the plane of the magnetic meridian, and at a certain distance

from the needle in such a way that the plane of the needle is unaltered by the magnet being removed or replaced when desired.

The wire to be experimented upon is represented by AA′. It is hung vertically at a distance of 10 centims. from, and due magnetic east of the magnetometer needle, by means of an arrangement of pulleys, P, P′, P″, and weights, W, W′, each weighing about half a kilogramme and attached to one end of the cords, T, T′, respectively as shown. In order that the wire may easily be detached from the cords, the other end of each cord, instead of being fastened directly to the end of the wire, is merely hooked, by a small brass hook which it carries, on to a loop of cord fastened to the end of the wire. The mode of fastening the loop of cord to the wire was as follows:—A cord 20 to 30 centims. long was made into a loop in such a way as to bring its ends together, and this latter part of the loop, after having been untwisted, was put over the end of the wire so as to enclose about 5 centims. of it in the untwisted portion, over which portion a thin string was tightly coiled a great number of times. This mode of fastening the cord to the wire allowed a heavy weight to be put on the wire without twisting or bending the latter in the slightest degree.

BB′ is the magnetising coil hung in such a manner from the string T that both its centre and axis coincide with those of the wire AA′. The coil used in the first part of the experiments was composed of only one layer of silk-covered copper wire wound on a straight brass tube of about 6 millims. in its internal diameter; the length of the coil was 108 centims., its radius was ·34 of a centimetre, and the number of turns of wire on the coil was 1,795, and the resistance of the coil including the electrodes was, at 14° C., 3·94 ohms. By means of this coil were obtained the results given in the columns headed 1 to 5 of the Table I. It was soon found that the coil just described was quite unsuitable for producing high magnetising forces, and that a modification was necessary. The coil, when modified, was 110 centims. long, and consisted of five layers of silk-covered copper wire laid on one above another; the radius of the innermost layer was ·340 of a centimetre, and that of the outermost was ·660 of a centimetre, and, therefore, the mean radius of the coil was ·50, and the mean distance between any two adjacent layers ·08 of a centimetre; the resistance of the coil, the electrodes included, was 30·8 ohms. at 14° C. The ends of the electrodes of the coil were permanently connected to the two terminals of a reversing key, K, the other two terminals of which were in connexion with the two electrodes of a Thomson tray battery so disposed that any desired number of cells, from 1 to 60 inclusive, could be placed in the circuit. A tangent galvanometer, G, was inserted in one of the connecting wires as shown in the sketch, so that whenever a current is passed through the

coil it was read and measured by this galvanometer. The weight W″ was simply used to balance the weight of the coil.

It will easily be seen from the arrangements of cords, pulleys, &c., that the wire, besides being kept straight, can be raised or lowered through any desired distance within a range of about 4 metres; and further, that when the wire is moved up or down the coil follows the movements, keeping its position with reference to the former unaltered. For the purpose of observing the position of the wire or the coil with great facility at any instant with reference to the line on a level with the magnetometer needle, there is provided, alongside the wire and coil, a scale divided into centimetres, and fixed to a wooden upright.

The orderly and systematic way in which the experiments were performed may be described generally thus:—A weight, the amount of which was different for different specimens of the wire as will be presently stated, was put on and taken off the wire, whilst the magnetising force was in action, about ten times in succession (this operation of successive application and removals of a weight will be hereafter called, for brevity, "ons and offs"), half a kilogramme being always on; then the wire, having been first placed so high up that its effect and that of the coil on the magnetometer was scarcely visible, was lowered 2 centims. by 2 centims., until it was so low down that little or no effect of the wire and coil was observable on the magnetometer, while the deflections of the magnetometer needle were noted for all the positions of the wire and coil. This process was followed in the case of all the wires, except the hard-tempered wire, and all the magnetising forces used, unless otherwise stated. It will be needless to enter into the discussion of the details of the object of subjecting the wire to the operations of "ons and offs," as they will be, I hope, shortly communicated to the Royal Society or elsewhere; suffice it to point out here that on commencing the preliminary experiments, it was soon discovered that in the first instance the wire was very irregularly magnetised, but that the effect of subjecting the wire, while under the influence of the vertical force, to the application and removal of a pull a certain number of times in succession, was to remove all the irregularities as to magnetisation, besides producing an enormous augmentation of its magnetism.

The results are given in the Tables I to IV. The general explanation of these and other accompanying tables is, that the "Distances" mean the distances of the centre of the wire from the level of the magnetometer needle, those distances measured from their level upwards being reckoned positive, and those measured downwards negative; while the "Deflections" mean the corresponding deflections of the needle in the scale-divisions—those deflections indicating the repulsion of the north-seeking pole, or red end of the needle, being

reckoned positive, and those indicating the attraction negative. The headings 1, 2, 3, &c., under "Deflections," are not only to show the order in which the experiments were performed, but to distinguish the results for one magnetising force from those for another; the exact value of the magnetising force in each case will be shown presently.

The first wire tried was a very soft iron (pure) wire,* supplied by Johnson and Nephew, Manchester, and is named in the table "Dark Wire," from its appearance. It was of No. 10 B.W.G., its breaking stress being about 15 kilogs. The piece experimented on was a metre long; its radius, when carefully calculated from its weight and specific gravity, was ·0374 of a centimetre, and therefore its sectional area was ·00439 square centim. The weight which was used for the operation of "Ons and Offs" was 8 kilogs., only with this exception, that at the beginning, while the force magnetising the wire was that due to the vertical component of the earth's magnetism alone, a weight of 10 kilogs. was put on once or twice. The wire underwent an elongation of 2·9 per cent. of its original length, so that it was now 102·9 centims., and its sectional area ·00425 square centim.; the elongation was permanent and constant, that is, the subsequent application of 8 kilogs. produced no more effect as to elongation. The results for this wire are shown in the Table I. In this table, the results under the heading numbered 1, which are those for the Glasgow vertical force, it must be mentioned, were obtained after the wire had been treated in the following manner:—The operation of "Ons and Offs," of a weight of 8 kilogs., having been performed while the wire was hanging one way, say, with the end A up, its magnetisation was observed in the manner explained before; the wire was then inverted, and the operation of "Ons and Offs" was again performed while it was hanging with the end A′ up, that is while the vertical force was acting in the opposite direction with respect to the wire, and its magnetisation was again observed; this process was repeated until the magnetisation of the wire in the two cases was equal, or nearly so, in intensity, but opposite in polarity. The first and second columns under any of the headings numbered 1 to 8 give the result obtained in the two cases respectively: (1) while a weight of 8 kilogs. was actually hanging on the wire (a case to be hereafter denoted by "On"), and (2) while the weight was off (a case to be hereafter denoted by "Off"); and the third column, if any, contains the result obtained for the effect of the coil alone carrying a current. The first column under 12 and 13 contains the result obtained (in the case "Off") immediately after reversing the current in the coil, the operation of "Ons and Offs" having been of course performed before the current was reversed; while the second and

* This wire is of the same kind as that used in the experiments described in Sir William Thomson's paper, "On the Electrodynamic Qualities of Metals, Part VII."

third columns contain the results obtained after the wire had been subjected to "Ons and Offs," when the reversed current was circulating through the coil, the former corresponding to the case "On" and the latter to the case "Off." The first column in the rest, that is, 14 to 17, is subject to the same explanation as the first column in 12 and 13; while the second column contains the result obtained in the same way as the third column in 12 and 13.

The next wire experimented on was also a pure soft iron wire, but not so soft as the last one; it is marked "Bright Wire"* in the tables. Its gauge is about No. 20 B.W.G., and its breaking stress is about 20 kilogs. The piece experimented on was also a metre long; its radius was ·0450 of a centimetre, and therefore its sectional area ·006362 square centim.; 12 kilogs. weight was employed for "Ons and Offs." The wire elongated 6·2 per cent. of its length, so that now its length was 106·2 centims., and remained so during all the rest of the experiment; the area of its cross-section being now ·00599 square centim. The Table II refers to this wire. As regards the first and second columns headed 1 under Deflection, exactly the same remark applies to this table as to the last table. The first and second columns under any of the headings give the results in the cases of "On" and "Off" respectively; and the third and fourth columns give the results (both in the case of "Off") obtained, the former immediately after reversing the current in the coil, and the latter after the operation of "Ons and Offs" had been performed while the reversed current was kept flowing through the coil.

The Table III contains the results for the "Steel Pianoforte Wire," which was of the same gauge as the "Dark Wire," and which is largely used in Sir William Thomson's sounding machines. The breaking weight of this wire is said to be roughly 100 kilogs. The length of the piece of the wire experimented on was a metre; its radius was ·03755 of a centimetre, and therefore the area of its cross-section was ·004452 square centim. A weight of 16 kilogs. was always used for "Ons and Offs." No elongation of the wire was observed. To both the first and second columns under all the headings in the Table III precisely the same remarks apply as to those in the preceding tables; while the third column, should there be one, gives the result for the coil alone.

The last of the thin wires experimented on was a glass-hard-tempered steel wire, the results of which are exhibited in the Table IV. The mode of tempering which was adopted is perhaps worthy of a passing notice. A convenient length was cut from the same hank as the preceding

* Further particulars regarding the elasticity, &c., of this wire are found in Mr. J. T. Bottomley's interesting paper on the "Effects of Long-continued Stress on the Elasticity of Metals," "Proc. Roy. Soc.," vol. xxix (1879), p. 221.

wire (pianoforte wire); and while held horizontally by means of pliers over a tray containing cold water, it was raised to a bright red heat by passing through it a strong current from a Faure battery, and suddenly plunged into the tray. This plan proved a complete success, the heat being equally distributed throughout the whole mass of the wire; the tempering was, of course, as uniform as it could be all over the length of the wire, perhaps, with the exception of the ends where it was held. When short pieces were cut off from the extremities, the wire was 78·42 centims. long; the area of its cross-section was now ·004326 square centim., the wire having lost nearly 2 per cent. of its weight by the process of tempering. This wire was, of course, so exceedingly brittle that the operation of "Ons and Offs" of a heavy weight was an impossibility, and consequently no weight was put on the wire at all, except those used to keep it vertically straight. With reference to the explanation of the Table IV, the first column in 1, 2, 3, &c., refers to the result arrived at when the magnetising force was kept acting on the wire; and the second column, if there be one, refers to the result arrived at directly after the withdrawal of all magnetising force, except that due to the vertical component of the earth magnetism.

Somewhat thick bars of cast iron, hard-tempered steel, and soft iron, were then procured and experimented upon, with a view to determine approximately the law of magnetisation of those bars, and to compare the results with each other and with those for the wires. The bars were nearly equal in their dimensions; they were all 61 centims. in length and very nearly square in section; the sectional area of the cast-iron bar, when calculated from its weight and specific gravity, was approximately ·950 square centim., that of the steel bar ·948 square centim., and that of the soft iron bar ·901 square centim.

With regard to the mode of experimenting in the case of these bars, though it remained the same in principle as before, it necessarily differed in details, which I proceed to describe thus:—In the first place, the coil employed for magnetising the bars was 68 centims. long, and consisted of three layers of insulated copper wire wound on a tube of copper nearly square, each layer containing 620 turns; the whole area inclosed by all the turns of wire per unit length was 89 sq. centims. approximately, though not very accurately on account of the difficulty of measuring exactly the dimensions of the coil, as it was not specially made for the purpose; and the resistance of the coil was about 3·78 ohms when cool. The bar to be experimented on was placed inside the coil, with its centre and axis coincident with those of the latter; and the whole arrangement thus fitted up was hung vertically in the same way as before by means of cords, pulleys, &c., with the common axis of the coil and the bar at a distance of 22 centims. from, and due magnetic east of, the magnetometer; the con-

nexions of the electrodes of the coil, the galvanometer, &c., being precisely the same as before.

The same procedure in experimenting as before was followed as far as possible; that is to say, the bar and the coil, having been placed high up to begin with, were lowered 2 centims. by 2 centims. until they were low down, while the deflections of the magnetometer needle were read for all the positions of the bar or the coil. In the case, however, where this procedure was hardly possible, or, at any rate, hardly worth going through, on account of the rapid variation of the current in the coil, arising partly from the heating up of the coil and partly from the polarisation of the battery (which consisted either of the Thomson tray, Daniell's, or of the Faure accumulators, the latter being chiefly used to obtain very high magnetising forces), the experiment was made in the following manner:—A point of the bar, 28 centims. distant from its centre, having been placed on a level with the magnetometer needle (as this position of the bar was such as to give the needle a maximum deflection for a high magnetising force), a strong current was allowed to pass through the coil, and as soon as the deflection of the needle was readable with a tolerable accuracy it was read off at a certain moment by one observer, while the strength of the current was measured by taking the reading of the galvanometer at the same moment by another observer on word of command from the former; the data thus obtained will, as we shall see, afford the means of determining approximately the magnetisation of the bar.

The results of experiments on the bar of cast iron, steel, and malleable iron, are given in the Tables V, VI, VII respectively, the general explanation of which has been already given in dealing with the other tables. The first column under any of the headings 1, 2, 3, &c., in each of the Tables V, VI, VII, contains the results obtained while the magnetising force was in action; while the second column, if there be one, contains the result obtained directly after the withdrawal of the force.

Now the best way to study the results given in all the Tables I to VII, is to plot curves in such a manner that the ordinates represent the "Distances" of the centre of the wire or bar from the datum line, the level of the magnetometer needle, and the abscissæ represent the "Deflections" of the needle in the scale divisions. To illustrate this, the results shown in the second and third columns under 7, Table I, are exhibited by the curves 1 and 2 respectively, Plate 10, in which those distances measured upward from the datum line are reckoned positive and those measured downwards negative; while those deflections indicating the repulsion of the red end of the needle are reckoned positive, and those indicating the attraction negative, according to the convention already adopted.

Table I.—Dark Wire.

Distances.	Deflections.																
	1.		2.		3.		4.		5.		6.*	7.			8.		
100	4	2	4	3	6	4	1	+ 0·5	1·5	1·5	2	2	2	1	2·5	2·5	1·5
90	6	3	8	6·5	10	9	2	1·5	2·5	2·5	4·5	5	5	2·5	5·5	5·5	3
80	10	4	19	16	25	23	4	3·5	5	5	9	11	11·5	5·5	13·5	13·5	8
70	28	12	59	45	79	69	10	9·5	14·5	15	26	34	34·5	18	42	43	24
68	34	18	74	60	104	93	14	13·5	19	22	36	46	47	25	56	57	32
66	43	22	92	78	134	120	18	17·5	25	27·5	48	59	60·5	32·5	73	74	41
64	55	30	118	106	182	170	24	23	34	37	63	81	82	45	97	98	57
62	71	38	149	138	236	223	31	30	43·5	46·5	84	104	106	58	125	126	74
60	96	50	202	186	319	290	41	40	56·5	61·5	110	135·5	138	74·5	162	167	98
58	125	68	271	2·9	438	390	54·5	53	73	80·5	139	171·5	174	91	203	208	118
56	166	96	359	334	526	502	69·5	68	93	102	165	200	203	101·5	235	240	134
54	218	124	440	418	702	596	83	81	110·5	117·5	180	215	217·5	102	248	253	134
52	266	152	504	450	783	646	91	91	119·5	124·5	179	211	214	92	241	244	120
50	318	172	548	463	805	652	91	90	118	121·5	165	191	193	75·5	214	217	98
48	342	184	536	407	749	601	81·5	81	104	107·5	140	160	161	60	176	177	78
46	348	186	476	325	643	465	67·5	66	85	86·5	110	124	125	47	135·5	136·5	56
44	330	164	403	219	494	332	51·5	49·5	65·5	67·5	82	95	96	35	102	103	44
42	298	140	323	120	369	219	39·5	37	50	52	65	72	73	27	76	77	34
40	260	104	250	62	276	140	28	26	38	38·5	47·5	50·5	51·5	21	56	57	25
38	220	80	191	40	214	109	21	20	30	30	37	40	40	16	44	45	19
36	178	61	151	18	153	71	16	14·5	23	23	28·5	30	30	12·5	33	34	15
34	148	45	110	12	120	58	12	11	19	19	23	24	24	9	26	26	13
32	114	30	85	5	88	46	9	8·5	15·5	15·5	19·5	18·5	18·5	7	20	20	11
30	92	20	69	4	69	33	7·5	7	12·5	12·5	16	15	15	6	17	17	10
28	74	11	52	2	51	26	6	5·5	10	10	13	12	12	4·5	13	13	8
26	60	5	42	2	39	18	4·5	4	7·5	7·5	11·5	9·5	9·5	3·5	10	10	7
24	45	0	31	1	27	12	3	2·5	5·5	5·5	10·5	7·5	7·5	3·5	7·5	7·5	6
22	37	0	24	1	16	5	1·5	1	4	4	7·5	6	6	3·5	6	6	6

* The first column under 6 being blank is omitted in printing.

Table I (continued).—Dark Wire.

Distances.	Deflections.																
	1.		2.		3.		4.		5.		6.*	7.			8.		
20	30	0	18	0	6	0	1	0	2·5	2·5	5	4·5	4·5	3	4·5	4·5	5·5
18	22	1	13	− 1	+ 1	− 5	0·5	−0·5	2	2	4	3	3·5	3	3	3	5
16	15	1	8	− 2	− 3	−11	0	−1	1	1	3·5	2	3	3	2	2	4
14	9	2	5	− 2	− 3	−12	0	−1	0·5	0·5	3	1·5	3·5	3	1·5	1·5	4
12	4	4	3	− 3	− 7	−13	0	−0·5	0	0	2·5	1	3	2·5	1	1	3
10	+ 1	5	4	− 4	− 3	−10	0	0	0·5	0·5	2	1	3	2·5	1	1	3
8	− 3	6	5	− 5	+ 1	− 6	0·5	0·5	2·0	2	2	1	3	2	1	1	3
6	− 5	7	6	− 5	+ 5	− 4	0·5	0·5	2·5	2·5	1·5	0·5	2·5	2	1	1	2
4	− 8	6	7	− 6	8	− 2	1·5	1	3	3	1·5	0	1	1	+ 0·5	+ 0·5	1
2	− 10	4	6	− 8	5	− 5	+0·5	0·5	2·5	2·5	0	− 1·5	+ 0·5	1	− 1	− 1	0·5
0	− 12	+ 2	+ 4	− 9	+ 5	− 9	0	0	+1	1	− 1	− 2	− 2	+0·5	− 2	− 2	0
− 2	− 13	0	0	−11	− 2	−12	0	−0·5	0	0	− 1·5	− 4	− 3	+0·5	− 5	− 5	−1
− 4	− 15	− 2	− 4	−12	− 7	−15	0	−1	−1	−1	− 2	− 5	− 5	0	− 7	− 7	−2
− 6	− 15	− 4	− 8	−12	−12	−17	−1·5	−1·5	−1·5	−1·5	− 2·5	− 6	− 6	−1	− 8	− 8	−2·5
− 8	− 15	− 7	−13	−11	−17	−19	−2	−2	−2	−2	− 3·5	− 7	− 7	−1	− 9	− 9	−3
−10	− 16	− 8	−15	−10	−18	−16	−2	−2	−2·5	−3·5	− 4	− 7·5	− 7·5	−1	− 9·5	− 9·5	−3·5
−12	− 16	− 9	−17	− 8	−20	−14	−2·5	−2	−3	−4·5	− 4·5	− 8	− 8	−1	−10	−10	−3·5
−14	− 17	− 8	−18	− 6	−19	−10	−2	−1·5	−3	−4	− 5	− 8·5	− 8·5	−1	−10·5	−10·5	−4
−16	− 18	− 7	−19	− 4	−19	− 6	−1·5	−1	−3	−4	− 5	− 9	− 9	−1·5	−11	−11	−4
−18	− 19	− 6	−23	− 3	−21	− 3	−1	−0·5	−2·5	−3·5	− 5	− 8·5	− 8·5	−2	−10·5	−10·5	−4·5
−20	− 21	− 7	−29	− 2	−23	− 1	−0·5	−0·5	−2	−3	− 5·5	− 8	− 8	−2	−10	−10	−5
−22	− 26	− 9	−28	− 3	−20	− 3	−1	−1	−1·5	−2·5	− 6·5	− 8	− 8	−2·5	−10	−10	−5·5
−24	− 31	− 11	−28	− 4	−15	− 5	−0·5	−1	−1	−2·5	− 7·5	− 8	− 8	−2·5	−10	−10	−6
−26	− 41	− 18	−27	−10	−20	−15	−1	−1	−2	−3·5	− 8·5	− 9	− 9	−3	−11·5	−11·5	−6·5
−28	− 54	− 24	−26	−15	−24	−26	−2	−2	−3	−4·5	− 9·5	−10	−10	−3	−13	−13	−7
−30	− 74	− 33	−41	−25	−42	−42	−4	−4	−5·5	−7·5	−14	−13	−13	−4	−16	−16	−8·5

* The first column under 6 being blank is omitted in printing.

Table I (continued).—Dark Wire.

Distances.	Deflections.																
	1.		2.		3.		4.		5.		6.*	7.			8.		
−32	− 96	− 43	− 60	− 35	− 61	− 60	− 6·2	− 6	− 8	− 11	− 17·5	− 16	− 16	− 5·5	− 20	− 20	− 10
−34	−127	− 56	− 87	− 54	− 93	− 81	− 9	− 9	− 12	− 15	− 22·5	− 23	− 23	− 8	− 27	− 27	− 13
−36	−159	− 71	−114	− 72	−125	−102	−12·5	−12·5	− 16	− 19·5	− 28	− 30	− 30·5	− 11	− 35	− 35	− 16
−38	−202	− 88	−160	−100	−187	−142	−18·5	−18	− 23	− 27	− 38	− 41	− 42	− 14	− 47	− 48	− 21
−40	−246	−106	−206	−128	−250	−184	−26·5	−25	− 30·5	− 35·5	− 49	− 52	− 55·5	− 18	− 60	− 62	− 25
−42	−294	−128	−280	−170	−352	−254	−37·5	−35·5	− 43	− 49	− 69	− 73	− 77	− 22	− 83	− 86	− 34
−44	−336	−150	−365	−226	−487	−340	−51·5	−49·5	− 58·5	− 67·5	− 89	− 94	−100	− 30	−107	−110	− 43
−46	−356	−166	−453	−282	−614	−442	−70	−68	− 80	− 89	−114	−121·5	−129·5	− 41	−136	−143	− 57
−48	−354	−169	−528	−340	−754	−554	−84·5	−82	−104	−110·5	−145	−161	−167·5	− 58	−176	−183	− 75
−50	−323	−160	−562	−386	−832	−622	−96	−93·5	−123	−128	−172	−192·5	−199	− 81	−215	−220	− 98
−52	−275	−134	−641	−375	−816	−608	−95·5	−93·5	−127	−131	−186	−214	−220	−101	−242	−247	−123
−54	−230	−102	−470	−332	−722	−548	−87·5	−84·5	−119	−122·5	−181	−211	−216	−112	−245	−246	−135
−56	−174	− 78	−388	−272	−590	−454	−73·5	−70·5	−104	−106	−160	−196	−196	−110	−227	−226	−135
−58	−130	− 57	−296	−204	−459	−352	−55·5	−54	− 82	− 84	−133	−165	−164	− 95·5	−191	−191	−119
−60	− 86	− 44	−222	−157	−343	−268	−42	−41	− 62	− 64	−104	−130	−127	− 75·5	−150	−150	− 98
−62	− 72	− 32	−166	−119	−255	−206	−31·5	−31	− 45·5	− 46·5	− 79	− 99	− 99	− 58	−114	−114	− 73
−64	− 56	− 26	−129	− 91	−198	−160	−24	−23·5	− 35	− 36	− 62	− 73	− 73	− 45	− 88	− 88	− 56
−66	− 41	− 20	− 96	− 70	−137	−118	−18	−17·5	− 25·5	− 25·5	− 46	− 54	− 54	− 32·5	− 64	− 64	− 41
−68	− 33	− 16	− 76	− 55	−106	− 91	−14	−13	− 20	− 20	− 36	− 42	− 42	− 25	− 49	− 49	− 32
−70	− 26	− 12	− 59	− 41	− 82	− 68	−10	− 9·5	− 15	− 15	− 26	− 32	− 32	− 18	− 36	− 36	− 24
−80	− 10	− 4	− 19	− 16	− 26	− 22	− 4	− 3·5	− 0·5	− 0·5	− 9	− 10	− 10	−5·5	− 12·5	− 12·5	− 8
−90	− 6	− 3	− 8	− 6·5	− 11	− 8	− 2	− 1·5	− 2·5	− 2·5	− 5	− 5	− 5	−2·5	− 5	− 5	− 3
−100	− 4	− 2	− 4	− 3	− 6	− 3	− 1	− 0·5	− 1·5	− 1·5	− 2	− 2	− 2	−1	− 2	− 2	− 1·5

* The first column under 6 being blank is omitted in printing.

Table I.—Dark Wire.

Distances.	Deflections.																
	9.	10.	11.	12.			13.			14.		15.		16.		17.	
100	Exactly the same as the second column under Heading 8.	Magnetising force used in 7, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in second column of 7.	Magnetising force used in 6, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in 6.	− 1	− 1	− 1	− 0·5	− 0·5	− 0·5	− 0	− 0·5	− 2	− 4	+ 1	− 1·5	1·5	0
90				− 1·5	− 1·5	− 1·5	− 1	− 1	− 1	− 0·5	− 1	− 5	− 10	+ 2	− 2·5	4	− 0·5
80				− 4	− 4·5	− 4·5	− 3	− 3·5	− 3	− 2	− 2·5	− 11	− 19	+ 5	− 6	8	− 2
70				− 14	− 14·5	− 14·5	−10	−11	−10	− 6	− 8	− 32	− 56·5	14	− 17	18	− 7
68				− 19	− 19	− 19	−13	−15	−13·5	− 8	−10	− 42	− 74	17	− 21	23	− 9·5
66				− 24	− 25	− 25	−16	−19	−17·5	−11	−13·5	− 54	− 95	22	− 26	29	−13
64				− 33·5	− 34	− 34	−22	−26	−24	−15	−18	− 74	−130	30	− 35	37	−18
62				− 42·5	− 43	− 45	−29	−34·5	−31	−20	−24	− 98	−171	39	− 46	49	−25
60				− 57	− 59	− 59	−39	−45	−42	−26	−31·5	−132	−229	52	− 60	67	−30
58				− 75	− 77	− 77	−50	−59·5	−53·5	−35	−42	−171	−300	70	− 81	92	−40
56				− 92	− 94	− 95	−60	−74·5	−68·5	−42	−51·5	−215	−382	97	−103	120	−50
54				−106	−107	−112	−71	−87·5	−82·5	−50	−61	−258	−465	127	−128	153	−59
52				−115	−115	−122	−80	−97	−92	−55·5	−68·5	−273	−520	156	−149	188	−66
50				−112	−114	−119	−76	−95·5	−91·5	−55·5	−69	−279	−536	181	−163	210	−71
48				− 99	− 99	−105	−73	−87	−85	−50	−63	−250	−501	179	−165	211	−69
46				− 76	− 78	− 86	−59	−71	−70	−40·5	−52	−195	−424	174	−156	193	−65
44				− 58	− 60	− 66	−46	−56	−55	−32	−41	−142	−341	160	−140	169	−57
42				− 44	− 46	− 50	−35	−42	−42	−24	−31	−104	−255	131	−119	138	−48
40				− 31	− 32	− 35	−25	−29·5	−29·5	−18	−23·5	− 72	−187	106	−100	109	−40
38				− 22	− 25	− 26	−18	−22	−22	−11·5	−17	− 53	−143	82	− 81	87	−32
36				− 15	− 18	− 18	−12	−15	−15	− 5·5	−12	− 36	−104	62	− 65	68	−25
34				− 10	− 13·5	− 14	− 9	−11	−11	− 4	− 9	− 25	− 76	47	− 51	54	−19
32				− 6	− 9·5	− 10	− 6	− 7	− 7	− 3	− 6	− 15	− 54	35	− 39	42	−15
30				− 5	− 7	− 7	− 4	− 5	− 5	− 2	− 4	− 11	− 39	26	− 30	32	−11
28				− 3	− 5	− 5	− 3	− 3	− 3	− 1·5	− 3	− 8	− 26	18	− 22	22	− 7
26				− 2·5	− 4·5	− 4	− 2·5	− 2·5	− 2·5	− 1	− 2	− 5	− 23	12	− 16	16	− 6
24				− 2	− 4	− 3	− 2	− 2	− 2	− 0·5	− 1·5	− 2	− 11	7	− 11	10	− 5
22				− 2	− 5	− 3	− 2	− 2	− 2	− 0·5	− 1	− 4	− 10	5	− 9	7	− 4·5

Table I (continued).—Dark Wire.

Distances.	Deflections.																
	9.	10.	11.	12.			13.			14.		15.		16.		17.	
20	Exactly the same as the second column under Heading 8.	Magnetising force used in 7, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in second column of 7.	Magnetising force used in 6, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in 6.	−2	−6	−3	−2	−2	−2	−0·5	−1	−6	−9	3	−8	5	−4
18				−3	−6	−3·5	−2·5	−2·5	−2·5	−0·5	−1	−9	−10	2·5	−8	5	−4
16				−4	−6	−4	−3	−3	−3	−0·5	−1·5	−11	−13	2	−7	4	−4
14				−4	−6	−4	−1·5	−3	−3	−0·5	−2·5	−12	−15	2	−7·5	3	−3·5
12				−4	−5	−4	−2	−3	−3	0	−2	−14	−18	2	−7	3	−3
10				−4	−4·5	−3·5	−1·5	−3	−3	0	−2	−16	−19	3·5	−5·5	4	−2
8				−3·5	−3	−3	−1	−2·5	−2·5	0	−1·5	−17	−18	5	−4	5	−1
6				−3	−2	−2	−1	−2	−2	0	−1	−13	−16	4·5	−3	4·5	0
4				−1	−1	−1	−1	−1·5	−1·5	0	−1	−8	−14	4	−2	4	+2
2				−0	−0·5	−0·5	−0·5	−1	−1	0	−0·5	−4	−10	3·5	−0·5	2·5	2·5
0				0	0	0	0	0	0	0	0	0	−6	3	+1	2	3
−2				+0·5	+0·5	+0·5	+0·5	+0·5	+0·5	0	+0·5	+2	−3	1	2·5	+0·5	3
−4				1	1	1	1	1	1	0	1	4	−3	1	4	0	3
−6				1·5	1·5	1·5	1	1	1	0	1	8	0	+0·5	4·5	−0·5	2·5
−8				2	1	2	1	1	1	0	0·5	11	+1	0	5	−1	2
−10				1·5	1	1·5	0·5	0·5	0·5	0	0·5	8	2	−0·5	3·5	−1	1
−12				1	1	1	0	0	0	0	0·5	+3	1	−1	2	−1	0
−14				1	0·5	1	0	0	0	0	0·5	−1	0	−2·5	2·5	−1·5	−0·5
−16				1	1	1	0	0	0	0	1	−6	0	−4	3	−1·5	−1
−18				1·5	0·5	1·5	0	0·5	0·5	0	1·5	−10	0	−4·5	3	−1·5	−1
−20				2	1	2	0·5	0·5	0·5	0	1	−15	0	−5	4	−1·5	0
−22				3	2	3	1	1	1	0	1	−18	+2	−5·5	7	−3	0
−24				3·5	3	4·5	1·5	1·5	2	0	1·5	−20	4	−6	9	−5	0
−26				4·5	4	5·5	2	3	3	0	2	−19	8	−9	15	−6·5	+2
−28				6	5·5	6·5	3	4·5	4	+0·5	3	−18	12	−12	21	−8	8
−30				7	9·5	9	5	6·5	6	2	4	−16	23	−18	29	−12	14

Table I (continued).—Dark Wire.

Distances.	Deflections.																
	9.	10.	11.	12.			13.			14.		15.		16.		17.	
−32	Exactly the same as the second column under Heading 8.	Magnetising force used in 7, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in second column of 7.	Magnetising force used in 6, when reversed, gave the same deflections in opposite direction (directly after the reversal) as those in 6.	9	12	11·5	7	9	8·5	3·5	5·5	−14	36	− 25	38	− 21	19
−34				13	15·5	15	9	12	11	4·5	7·5	−10	52	− 34	49	− 31	23
−36				16·5	19·5	19·5	11	15·5	14	5·5	10	− 4	70	− 46	62	− 42	29
−38				23	27	27	16	21·5	20	9	15	+12	104	− 65	77	− 61	36
−40				30	35	35	21	28	26	13	20·5	+26	144	− 86	93	− 83	42
−42				45	49	49	31	40	37	21	30	79	218	−116	111	−120	47
−44				61	65	65	42	52	49	30	40	136	311	−149	132	−162	56
−46				78	82	83	57	67	64	41	54·5	196	420	−178	149	−204	64
−48				97	101	102	72·5	84·5	80	52	66	273	510	−196	166	−238	70
−50				112	114	117	76	92	90	59	74	321	561	−197	170	−247	68
−52				116	116	122	83	94	93	60	73	326	550	−180	158	−228	66
−54				108	107	114	73	84	86	57	66	300	493	−147	136	−192	57
−56				95	94	100	65	71	73	47	58	257	410	−114	110	−156	49
−58				77	77	82	52	58	59	37	46	206	325	− 83	87	−116	39
−60				60	60	63	40	44	45	28	35	160	242	− 60	67	− 86	31
−62				45	45	47	30·5	33	33·5	21	26	119	182	− 43	50	− 63	23
−64				34	35	36	23	25	26	16	20	91	141	− 33	38	− 50	17
−66				25·5	26·5	26	17·5	18·5	19	12	15	68	104	− 25	28	− 37	13
−68				19	21	20·5	13	14·5	15·5	9	11	52	80	− 20	22	− 28	10
−70				14·5	14·5	15	10	11	11·5	6·5	8	39	60	− 15	18	− 20	7
−80				4·5	4·5	5	3	3·5	3·5	2·5	2·5	14	20	− 5	8	− 8	2
−90				1·5	1·5	1·5	1·5	1·5	1·5	1	1	6	11	− 2	3·5	− 4	0·5
−100				1	1	1	1	1	1	0	0·5	3	4	− 1	1·5	− 1·5	0

Table II.—Bright Wire.

Distances.	Deflections.																	
	1.				2.*		3.				4.				5.			
100	3	1·5	1	− 1·5	0·5	0	1	1	0	− 0·5	1	1	0	− 1	1·5	1·5	− 1	− 1·5
90	6	3·5	3	− 3·5	1	0·5	2	2	0	− 1	2	2	− 0·5	− 2	3	3	− 2	− 3
80	14	8	6	− 8	3	1·5	4·5	4	0·5	− 3	5	5	− 2	− 5	7	7	− 5·5	− 7
70	40	29	24	− 29	8	4	14	12·5	1	−10·5	18	17	− 8	− 17	22·5	22·5	− 18·5	− 22·5
68	50	38	32	− 38	10	5	19	16	1	−14	24	23	−11	− 22	30	30	− 25	− 30
66	64	49	42	− 49	13	6·5	24	21·5	1·5	−18·5	31	30	−15	− 29	39·5	39·5	− 32·5	− 39
64	86	68	52	− 68	17	8·5	34	29	1·5	−25	42	40	−20	− 39	54	54	− 45	− 53
62	118	88	66	− 88	23	11	44	39	2	−33	56	52	−26	− 51	71	71	− 59·5	− 70
60	152	120	87	−120	30·5	15	60	51	2·5	−45	75	70	−35·5	− 69	94·5	95	− 79	− 94
58	196	159	112	−156	40	19·5	79	68	4	−58·5	98·5	93·5	−46·5	− 90·5	123	125	−103	−124
56	246	202	141	−200	51	25	96	82	5·5	−61·5	120·5	115	−56	−111	149	151	−121	−149
54	306	246	175	−246	59·5	31	108	93	9·5	−84	136	131·5	−62·5	−127	165	170	−139	−166
52	346	268	199	−266	66	36	119	102	14·5	−91	140	137	−61	−132	164·5	171	−142	−168
50	363	273	210	−272	66	38	113	97	19	−88	128	126·5	−61	−121	146	154	−125	−152
48	357	243	199	−242	60·5	37	97	82	23	−75·5	105	104	−35	−100	117	122	− 95	−121
46	336	211	171	−210	51·5	34·5	77	64	26	−60·5	79·5	77·5	−18	− 74	85	90	− 65	− 90
44	289	174	148	−174	41	29·5	59	46	26·5	−44·5	59	54·5	− 3	− 50·5	63	64	− 43	− 64
42	247	138	124	−138	31·5	25	52	30	25	−31	40	35	+ 6	− 33	41	42	− 21	− 42
40	196	102	93	−102	24·5	20	29	20	21·5	−22	27·5	23	+10·5	− 21	31	30	− 11	− 30
38	159	86	78	− 86	19	16	22	15	18	−16·5	21	17	+10	− 16	24	23	− 7·5	− 23
36	127	72	65	− 72	14·5	13	16	11	14·5	−12	15	12	10	− 11	18·5	17	− 4·5	− 17
34	103	64	56	− 64	11	10	12·5	9	11·5	− 9	12	10	8	− 8·5	15	14	− 4	− 14
32	81	56	49	− 56	9	8	9·5	7·5	9·5	− 7	9·5	8	6	− 6·5	12·5	11·5	− 3	− 11·5
30	70	48	42	− 48	7	6·5	7·5	6	7·5	− 6	7·5	6·5	5	− 6	9·5	9·5	− 2·5	− 9·5
28	60	40	36	− 40	5·5	5	6	5	5·5	− 5	6	5·5	4	− 5	7·5	8	− 2	− 8
26	54	36	31	− 36	5	4·5	5	4	5	− 4	5	5	3	− 4·5	7	7	− 2	− 7
24	48	31	26	− 31	4·5	4	4·5	3·5	4	− 3·5	4·5	4·5	2	− 4	6	6	− 2	− 6
22	46	29	23	− 29	4	4	4	3·5	3	− 3·5	4	4·5	1	− 4	5	5	− 2·5	− 5

* The first column under 2 being blank is omitted in printing.

Table II (continued).—Bright Wire.

Distances.	Deflections. 1.				2.*		3.				4.				5.			
20	43	27	20	− 27	4	3·5	3	3	2·5	− 3	3	4·5	0	− 4	4·5	4·5	− 3	− 4·5
18	41	26	19	− 26	3·5	3	2	2	2	− 2	2·5	4	0	− 4	4	4	− 3	− 4
16	41	26	19	− 26	3·5	3	1·5	2	1	− 2	2	4	− 1	− 4	3·5	3·5	− 3·5	− 3·5
14	39	27	20	− 26	3	2·5	1·5	2·5	1	− 2·5	1·5	3·5	− 1·5	− 3·5	3·5	3·5	− 3·5	− 3·5
12	39	28	22	− 27	3	2	1	3	0·5	− 3	1·5	3	− 2	− 3	3	3	− 3·5	− 3
10	40	29	* 22	− 28	2·5	2	1	3·5	0·5	− 3·5	1	3	− 2	− 3	3	3	− 3	− 3
8	40	29	23	− 29	2·5	1·5	1	3·5	0·5	− 3·5	1	3	− 2·5	− 3	1·5	3	− 3	− 3
6	40	31	25	− 30	2·5	1·5	0·5	3·5	0·5	− 3·5	1	3·5	− 2·5	− 4	1·5	3	− 3·5	− 3
4	42	34	28	− 32	2·5	1·5	0·5	3·5	0	− 3·5	0·5	4	− 2	− 4	1·5	3	− 4	− 3
2	44	37	30	− 35	3	1·5	0·5	4	0	− 4	0·5	4	− 1·5	− 3·5	1	3	− 3·5	− 3
0	45	40	32	− 38	3	2	0	4	0	− 4	0	4	− 1	− 4	1	3	− 3	− 3
− 2	44	42	35	− 40	3	1	− 0·5	2	2	− 4	0	4	+ 2	− 4	0·5	2	− 1	− 2
− 4	42	45	38	− 43	3	2·5	− 0·5	4·5	3	− 4·5	− 0·5	4	4	− 4	0	1·0	0	− 1
− 6	40	44	37	− 42	2	2	− 1	3	3·5	− 3	− 1·5	2·5	6	− 2·5	− 0·5	0·5	+ 3	− 0·5
− 8	36	43	37	− 42	1·5	1·5	− 2	1·5	4·5	− 1·5	− 2	1·5	8	− 1·5	− 1	0	7	0
−10	30	37	32	− 37	0	0	− 2	0	4	0	− 2·5	0	8·5	0	− 2	− 0·5	8	+ 0·5
−12	24	31	28	− 32	− 1	− 1	− 3	− 1·5	3	+ 1·5	− 3·5	− 1·5	9	+ 1·5	− 3	− 1	9·5	1
−14	18	21	19	− 21	− 2	− 2	− 3	− 2·5	1	2	− 3·5	− 2·5	6	3	− 3·5	− 2	7·5	2
−16	+ 10	+ 11	+ 10	− 11	− 3·5	− 3	− 3	− 4	− 1·5	3·5	− 4	− 4	+ 3	4·5	− 4	− 2·5	5·5	3
−18	− 2	− 4	− 1	+ 4	− 4	− 4	− 3	− 4·5	− 3·5	3·5	− 3·5	− 4·5	− 1	5·5	− 4	− 3·5	+ 1·5	3·5
−20	− 16	− 18	− 13	+ 17	− 5	− 5	− 3	− 5	− 5·5	4	− 3	− 5	− 6	6·5	− 3·5	− 4·5	− 2	4·5
−22	− 38	− 33	− 24	33	− 5·5	− 5	− 3·5	− 6	− 7	5	− 3·5	− 5·5	− 7·5	7	− 6	− 5·5	− 2·5	5·5
−24	− 58	− 49	− 36	48	− 5·5	− 5·5	− 4	− 6·5	− 8·5	6	− 3·5	− 6	− 9	8	− 7	− 6·5	− 3	6·5
−26	− 79	− 59	− 47	59	− 6	− 5·5	− 5	− 7	− 8·5	6·5	− 4·5	− 6·5	− 8·5	8·5	− 8	− 8	− 1	8
−28	−111	− 70	− 58	70	− 6·5	− 6	− 6	− 7	− 9	7	− 6	− 7	− 8·5	8·5	− 9	− 9·5	0	9·5
−30	−140	− 78	− 62	78	− 8	− 7·5	− 7	− 7·5	− 9·5	7·5	− 8	− 8	− 8	9	− 9·5	− 1	+ 1	10

* The first column under 2 being blank is omitted in printing.

Table II (continued).—Bright Wire.

Distances.	Deflections. 1.				2.*		3.				4.				5.			
− 32... ...	−171	− 87	− 67	87	− 9·5	− 9	− 10	− 8	−10	8·5	− 11	− 9	− 8	9·5	− 11	− 12	2	12
− 34... ...	−189	− 94	− 71	94	−12	−10·5	− 14	− 9·5	−12	10	− 15	− 10·5	− 8·5	10·5	− 13·5	− 15	3·5	15
− 36... ...	−210	−104	− 75	103	−15	−12·5	− 18	− 11·5	−14·5	12	− 19	− 12·5	− 9	12	− 17·5	− 19	5	19
− 38... ...	−227	−114	− 85	114	−19	−16	− 26	− 16·5	−17·5	18	− 26	− 18	− 8·5	18	− 25	− 27	10	26
− 40... ...	−237	−125	− 95	126	−24	−20	− 34	− 22·5	−21	23	− 34	− 25	− 8	24	− 32	− 35	15	33·5
− 42... ...	−259	−152	−112	150	−32	−24	− 49	− 33	−24	33	− 48	− 38	− 2	37	− 48	− 50	26	49
− 44... ...	−289	−186	−150	184	−42	−30	− 65	− 49	−26	46	− 68	− 57	+ 6	56	− 68	− 70	46	69
− 46... ...	−319	−224	−186	225	−52	−35	− 85·5	− 67	−26	63	− 90	− 81	19	79·5	− 95	− 96	69	95
− 48... ...	−359	−264	−223	265	−61	−38	−106	− 87·5	−24	80	−120	−106	39	105	−126	−129	101	128
− 50... ...	−389	−306	−251	306	−66·5	−39	−120	−101·5	−20	91·5	−137	−127	53	125	−152	−156	126	153
− 52... ...	−373	−311	−250	312	−66	−36	−121·5	−105	−14·5	94·5	−143	−136	64·5	134	−166	−169	140	168
− 54... ...	−340	−290	−233	292	−61	−31·5	−110	− 99	− 9	87·5	−134	−130	64	127	−160	−165	137	163
− 56... ...	−287	−248	−195	245	−52	−26	− 93·5	− 85	− 5·5	74	−114	−114	58	110·5	−142	−149	123	146
− 58... ...	−237	−197	−162	196	−41·5	−20	− 73·5	− 67	− 3·5	60·5	− 91	− 91	47·5	89	−114	−121	100	119
− 60... ...	−182	−148	−124	150	−31	−15	− 55	− 56·5	− 2	45	− 70	− 70	36	68	− 86	− 92	76	90
− 62... ...	−138	−112	− 96	111	−23·5	−11	− 41	− 33	− 1·5	34	− 51	− 51·5	27	50	− 63·5	− 69	59	68
− 64... ...	−109	− 85	− 77	85	−18	− 8·5	− 32	− 29	− 1·5	26	− 40	− 40	21	39	− 48	− 53	44	53
− 66... ...	− 83	− 65	− 59	65	−13·5	− 6·5	− 24	− 21	− 1·5	19	− 30	− 30	15	28	− 36	− 39	32·5	39
− 68... ...	− 65	− 50	− 47	49	−10	− 5	− 19	− 16	− 1	14·5	− 24	− 24	11	22	− 29	− 30	24	30
− 70... ...	− 43	− 38	− 35	38	− 8	− 4	− 14	− 12·5	− 1	10·5	− 18	− 18	8	17	− 22·5	− 22·5	18·5	23·5
− 80... ...	− 15	− 12	− 10	12	− 3	− 1·5	− 4	− 4	− 0·5	3	− 5	− 5	2	5	− 7	− 7·5	5·5	7·5
− 90... ...	− 8	− 6	− 5	6	− 1	− 0·5	− 2	− 2	0	1	− 2	− 2	0·5	2	− 3	− 3	2	3
−100... ...	− 4	− 3	− 2	3	− 0·5	− 0	− 1	− 1	0	0·5	− 1	− 1	0	1	− 1·5	− ·5	1	1·5

* The first column under 2 being blank is omitted in printing.

Table II.—Bright Wire.

Distances.	Deflections. 6.				7.*			8.				9.*		
100	2	2	− 1·5	− 2	2	− 2	All the deflections the same as those in the second column, 7, with their signs changed.	2·5	2·5	All the deflections the same as those in the last column, with their signs changed.	All the deflections the same as those in the last column, with their signs changed.	2·5	− 2·5	All the deflections the same as those in the last column, with their signs changed.
90	4	4·5	− 3·5	− 4·5	5	− 5		5·5	5·5			6	− 6	
80	8	9	− 7	− 9	11·5	− 11·5		14	14			15	−15	
70	28	29	− 26	− 28·5	37·5	− 37·5		43·5	44			50	−50	
68	37	38	− 36	− 36	50	− 49·5		59	59			66	All the deflections the same as those in the last column, with their signs changed.	
66	48	49·5	− 47	− 46	65	− 64·5		76	76·5			86		
64	66	68	− 66	− 67	90	− 89		102	103			117		
62	88	91	− 88	− 90	119	−117		138	141·5			159		
60	115	120	−116	−117	155	−155		181	136·5			208		
58	151	156	−151	−153	199	−199		227	240			265		
56	180	187	−178	−184	235	−235		265	278			307		
54	196	205	−196	−202	258	−257		276	300			327		
52	192	202	−195	−199	251	−249		264	287			307		
50	163	175	−171	−174	215	−214		224	244			261		
48	126	136	−133	−135	165	−164		174	189			202		
46	91	100	− 95	−100	120	−120		125	138			146		
44	65	70	− 67	− 70	86	− 86		89	98·5			104		
42	43	47	− 45	− 48	60·5	− 60·5		63	70			76		
40	31	35	− 31	− 35	43	− 43		44	49			56		
38	24	27	− 24	− 27	33	− 33		35	39			43		
36	17·5	20	− 18	− 20	24·5	− 24·5		27	29			32·5		
34	14	16	− 14	− 16	19	− 19		22	23			26		
32	11	13	− 11·5	− 13	15	− 15		17	18			20		
30	9	11	− 9	− 11	12·5	− 12·5		14	15			16		
28	7	9·5	− 8	− 9·5	10	− 10		11	12			14		
26	6·5	8·5	− 6·5	− 8·5	9	− 9		10	11			12		
24	5·5	7·5	− 5·5	− 7·5	8·5	− 8·5		9	9·5			10·5		
22	4·5	7	− 5	− 7	8	− 8		8·5	8			9·5		

* The first columns under 7 and 9 being blank are omitted in printing.

Table II (continued).—Bright Wire.

Distances.	Deflections.													
	6.				7.*			8.				9.*		
20	4	5·5	− 5	− 5·5	7·5	− 7·5	All the deflections the same as those in the second column, 7, with their signs changed.	8	7·5	All the deflections the same as those in the last column, with their signs changed.	All the deflections the same as those in the last column, with their signs changed.	9	All the deflections the same as those in the last column, with their signs changed.	All the deflections the same as those in the last column, with their signs changed.
18	3·5	4·5	− 4	− 4·5	7	− 7		7	5·5			8·5		
16	3	4	− 4·5	− 4	6	− 6		6·5	5			8·5		
14	3	3·5	− 4	− 3·5	5·5	− 5·5		6	4·5			8		
12	2·5	2·5	− 3·5	− 2·5	5	− 5		6	4·5			8		
10	2·5	3	− 3	− 3	4·5	− 4·5		5·5	4			7		
8	2	3	− 3	− 3	4	− 4		5	3·5			6		
6	2	3·5	− 3·5	− 3·5	4	− 4		4	3			6		
4	2	3·5	− 3·5	− 3·5	4·5	− 4·5		4	3			5·5		
2	1·5	3·5	− 3·5	− 3	4	− 4		3	2			5		
0	1	3	− 4	− 3	4	− 4		2	2			4		
− 2	0·5	3	− 3	− 1·5	2·5	− 2·5		1	1·5			3		
− 4	0	2	− 2	0	1·5	− 1·5		0	0·5			2		
− 6	− 0·5	1·5	− 0·5	0	0·5	0·5		0	0			1		
− 8	− 0·5	+ 0·5	+ 1	+ 0·5	0	0		− 0·5	− 0·5			0		
−10	− 1	− 0·5	1·5	1	− 1	− 1		− 0·5	− 1			− 0·5		
−12	− 2	− 1·5	2	2	− 2	− 2		− 1	− 1·5			− 1		
−14	− 2·5	− 2	2·5	2·5	− 3	− 3		− 1	− 2·5			− 1·5		
−16	− 3	− 3	3	3	− 3·5	− 3·5		− 1·5	− 3			− 1·5		
−18	− 3·5	− 4	4	3·5	− 5	− 5		− 2·5	− 4·5			− 3·5		
−20	− 4·5	− 5	4·5	4·5	− 6	− 6		− 4	− 5·5			− 5		
−22	− 5·5	− 6	5·5	6	− 8	− 8		− 6·5	− 8			− 8		
−24	− 7	− 7·5	6	7	−10	−10		− 9	−10·5			−11		
−26	− 8·5	− 9·5	7·5	8·5	−12	−12		−12	−12·5			−13		
−28	−10	−11	9	10	−13·5	−13·5		−14	−14·5			−15		
−30	−11·5	−13·5	10·5	11	−16	−16		−16	−16			−18		

* The first columns under 7 and 9 being blank are omitted in printing.

Table II (continued).—Bright Wire.

Distances.	Deflections.													
	6.				7.*			8.				9.*		
− 32... ...	− 13	− 14·5	12	12·5	− 18	− 18	All the deflections the same as those in the second column, 7, with their signs changed.	− 18	− 19	All the deflections the same as those in the last column, with their signs changed.	All the deflections the same as those in the last column, with their signs changed.	− 21	All the deflections the same as those in the last column, with their signs changed.	All the deflections the same as those in the last column, with their signs changed.
− 34... ...	− 16·5	− 18·5	15·5	16	− 22	− 22		− 23	− 24			− 26		
− 36... ...	− 20	− 22	19	20	− 27	− 27		− 28	− 29			− 31		
− 38... ...	− 28	− 29	25·5	27·5	− 35	− 34		− 39	− 39			− 42		
− 40... ...	− 36	− 37	32	35	− 45	− 45		− 50	− 49·5			− 54		
− 42... ...	− 53	− 53	48	51	− 62·5	− 62·5		− 70	− 69			− 76		
− 44... ...	− 74	− 75	72	73	− 88·5	− 88·5		− 97	− 96			−106		
− 46... ...	−105	−106	102	104	−122	−122		−134	−134·5			−146		
− 48... ...	−140	−141	138	138	−167	−166		−193	−197			−206		
− 50... ...	−173	−175	172	172	−209	−208		−240	−246			−264		
− 52... ...	−193	−198	191	196	−245	−243		−279	−288			−317		
− 54... ...	−191	−197	192	195	−252	−251		−286	−292			−336		
− 56... ...	−174	−182	176	180	−236	−236		−264	−276			−317		
− 58	−142·5	−151	142	149	−200	−199		−221	−233			−272		
− 60... ...	−110	−115	110	114	−155	−154		−172	−185			−212		
− 62... ...	− 82	− 86	82	85	−118	−117		−129	−141			−159		
− 64... ...	− 63	− 67	63	66	− 89	− 89		− 96	−103			−118		
− 66... ...	− 46	− 49	46	48	− 68	− 66		− 74	− 78			− 88		
− 68... ...	− 36	− 38	36	38	− 47	− 47		− 57	− 59			− 67		
− 70... ...	− 27	− 29	26	28·5	− 37·5	− 37·5		− 43·5	− 44			− 50		
− 80... ...	− 7·5	− 9	7	9	− 11·5	− 11·5		− 14	− 14			− 15		
− 90... ...	− 3·5	− 4·5	3·5	4·5	− 5	− 5		− 5·5	− 5·5			− 6		
−100... ...	− 1·5	− 2	1·5	2	− 2	− 2		− 2·5	− 2·5			− 2·5		

* The first columns under 7 and 9 being blank are omitted in printing.

Table III.—Steel Pianoforte Wire.

Distances.	Deflections.															
	1.		2.		3.		4.		5.		6.		7.*		8.*	
100	0	0	0·5	0·5	2	2	0·5	0·5	1	1	1·5	1·5	2	1	2·5	1·5
90	0·5	0·5	2	2	5	5	1	1	2	2	3	3	4	2·5	5	3
80	1	1	5	5	12	12	2	2	5	5	8	8	10	5·5	12	8
70	2·5	2·5	18	19	38	39	4·5	4·5	15	15	24	24	30	18	36	24
68	3	3	25	25	52	53	6·5	6·5	20	20	30	30	40	25	47	32
66	4	4	32	33	67	69	8·5	8·5	27	27	39	39	51	32·5	60	41
64	5	5	45	45	92	99	11	11	37	37	54	54	71	45	82	57
62	6·5	6·5	58	59	118	123	14	14·5	48	48	71	72	93	58	110	74
60	10	10·5	75	78	157	166	18·5	18·5	62	63	84	85	129	74·5	142	98
58	13	13·5	97	102	209	215	25	25·5	81	83·5	120	122	154	91	185	119
56	17·5	18	123	128	255	265	33	33·5	101	105	143	148	183	101·5	222	135
54	22	22	143	152	302	314	42	42·5	119	125·5	163	171	208	102	250	135
52	27·5	28	156	166	332	348	53	53·5	132	141	178	183	217	92	254	121
50	30·5	30·5	159	172	342	371	61·5	62	137	147	178	183	208	75·5	242	99
48	31	31	143	157	327	357	63	65	132	140	161	167	189	60	217	79
46	29	30	127	134	286	311	59	62	112	122	132	140	157	47	173	58
44	25·5	26	106	111	236	262	51	55	89	100	103	111	124	36·5	134	46
42	21·5	21·5	82	86	180	203	41	43·5	68	78	77	85	94	28·5	103	36
40	19	19	59	63	141	162	30·5	35	50	57	55·5	64	71	23	73	26
38	15·5	16	46	50	111	127	24·5	27·5	39	44	43	50	54	18	64	21
36	12·5	13	34	37	83	95	18	20	27	32	31	37	40	13·5	48	17
34	10·5	11	27	30	65	75	14	16	22	25·5	24	29	31	10	33	15
32	8·5	9·5	22	24	49	57	10	11·5	16·5	19·5	18	22	25	8	24	13
30	8	9	17	20	39	46	7·5	9	12	16	14	18	20	7	16	12
28	7·5	7·5	14	16	29	36	6·5	7	9·5	10·5	11	15	16	5·5	14	10
26	7·5	7·5	12	14	24	28	5·5	6	7·5	9	9	13	14	4·5	11	8
24	7·5	7·5	11	13	19	21	4·5	4·5	6·5	7·5	7·5	11	12	4·5	9	7
22	7	7	9	11	15	16	4	4	5·5	6	6·5	10	11	4	8·5	6·5

* The first columns under 7 and 8 being blank are omitted in printing.

Table III (continued).—Steel Pianoforte Wire.

Distances.	Deflections.															
	1.		2.		3.		4.		5.		6.		7.*		8.*	
20	6	6·5	7	9	12	12	4	4	4	5	6	9	9	4	8	6
18	6	6·5	6	7	8	8	3	3	3·5	4·5	5·5	8·5	8	4	7·5	5·5
16	5·5	6	5	7	3	3	2	2	3·5	4	5	8	8	4	7	5
14	5·5	6	4	6	0	−1	1·5	1·5	3	3·5	5	7·5	7	4	6·5	4·5
12	5	5·5	3	5	−4	−5	1	1	2·5	3	4·5	7	6	4	6	4
10	4·5	4·5	2·5	3·5	−5	−7	0	0	2	2·5	4	6·5	5	4	5·5	3·5
8	3	3	2	2·5	−6	−8	−0·5	−0·5	2	2	4	6	5	3	5	3
6	1·5	1·5	1	2	−7	−9	−1	−1	2	1·5	3·5	5	4	3	4	2
4	0	0	0	0	−10	−11	−1	−1	1·5	1	3	4	3·5	2	3	1
2	−0·5	−0·5	0	0	−6	−8	−1	−1	1	0	2	3·5	3	2	2	0·5
0	−1	−1	0	0	−4	−6	−1	−1	0	0	1	3	3	2	0	0
−2	−2	−2	0	0	−2	−4	−0·5	−0·5	−0·5	0	0	1·5	1	1	0	−1
−4	−2·5	−2·5	1	1	0	−5	−0·5	−0·5	−1	0	0	0	0	0	−0·5	−2
−6	−3	−3	0	0	−1	−5	−0·5	−0·5	−1·5	−0·5	−0·5	−0·5	−1	−1	−1	−2·5
−8	−3·5	−3·5	−1	−1	−2	−4	−0·5	−0·5	−2	−1	−1	−1	−2	−1	−2	−3
−10	−3·5	−3·5	−0·5	−0·5	−1	−4	−0·5	−0·5	−2·5	−2·5	−1·5	−1·5	−2·5	−1	−2	−3·5
−12	−3·5	−3·5	0	0	−1	−3	−0·5	−0·5	−3	−3	−3	−3	−3	−1	−3	−3·5
−14	−3·5	−3·5	−0·5	−0·5	−2	−4	−0·5	−0·5	−3	−3	−3	−3	−3	−1	−4·5	−4
−16	−3·5	−3·5	−1	−1	−2	−5	−0·5	−0·5	−3	−3	−3	−3	−3	−1·5	−6	−5
−18	−3·5	−3·5	−3	−3	−3	−7	−1	−1	−3·5	−4	−4	−4	−3·5	−2	−7	−5·5
−20	−3·5	−3·5	−5	−5	−4	−10	−1·5	−1·5	−4	−5	−5	−5	−5	−2	−8	−6
−22	−3	−3	−9	−9	−9	−18	−2·5	−2·5	−5	−6	−5·5	−5·5	−6	−2·5	−9	−6·5
−24	−3	−3	−13	−13	−14	−27	−4	−4	−6	−7	−7	−7	−7	−3·5	−10	−7
−26	−4	−4	−18	−18	−23	−36	−5·5	−5·5	−7·5	−8·5	−9	−9·5	−9	−4	−13	−7·5
−28	−5	−5	−23	−23	−33	−46	−7·5	−7·5	−9	−10	−11	−12	−13	−4	−16	−8
−30	−4	−4	−27	−29	−45	−59	−10	−10·5	−13	−14	−15	−16	−17	−5·5	−21	−9·5

* The first columns under 7 and 8 being blank are omitted in printing.

Table III (continued).—Steel Pianoforte Wire.

Distances.	Deflections.															
	1.		2.		3.		4.		5.		6.		7.*		8.*	
−32......	−4·5	−4·5	−31·5	−34	−59	−73	−13·5	−14	−17	−18	−20	−21	−22	−7	−27	−11
−34......	−6	−6	−37	−41	−76	−90	−17	−18	−23	−24	−28	−30	−28	−10	−36	−14
−36......	−7·5	−7·5	−44	−48	−95	−108	−21·5	−23	−30	−31	−35	−37	−38	−13	−45	−17
−38......	−10	−10	−53	−59	−120	−134	−27	−29	−42	−42	−47	−50	−48	−16	−60	−22
−40......	−13·5	−13·5	−64	−72	−146	−163	−33	−35	−54	−54	−60	−64	−61	−19	−80	−26
−42......	−18	−19	−81	−90	−181	−195	−41	−43	−71	−74	−81	−86	−88	−23	−105	−35
−44......	−24	−25	−105	−114	−219	−242	−50	−52	−91	−96	−103	−108	−119	−32	−140	−45
−46......	−31	−32	−130	−140	−248	−287	−56	−58	−112	−119	−128	−132	−153	−43·5	−178	−60
−48......	−36	−37	−153	−164	−314	−329	−61	−62	−129	−138	−163	−167	−191	−59	−215	−78
−50......	−38	−39	−162	−174	−330	−346	−59	−60	−136	−145	−178	−183	−217	−81	−242	−100
−52......	−36	−37	−159	−172	−320	−337	−50·5	−52	−131	−140	−179	−184	−228	−101	−253	−125
−54......	−28	−29	−146	−156	−291	−307	−40·5	−42	−118	−125	−168	−175	−216	−112	−242	−137
−56......	−23	−24	−128	−136	−250	−262	−31	−32·5	−101	−106	−147	−153	−193	−110	−216	−137
−58......	−17	−18	−107	−111	−207	−217	−24	−25	−83	−87	−121	−127	−158	−95·5	−180	−121
−60......	−15	−15	−82	−84	−159	−167	−17·5	−18	−64	−66	−95	−100	−121	−75·5	−140	−99
−62......	−9·5	−9·5	−63	−65	−123	−127	−14	−14·5	−48	−49	−72	−77	−90	−58	−107	−74
−64......	−7·5	−7·5	−48	−50	−95	−99	−11	−11·5	−37	−38	−55	−60	−66	−45	−79	−57
−66......	−6	−6	−35	−37	−68	−72	−8·5	−8·5	−27	−27	−39	−44	−50	−32·5	−58	−41
−68......	−4·5	−4·5	−27	−28	−53	−55	−6·5	−6·5	−21	−21	−31	−35	−37	−25	−45	−32
−70......	−3	−3	−20	−20	−38	−39	−4·5	−4·5	−15	−15	−24	−25	−30	−18	−35	−24
−80......	−1	−1	−6	−6	−12	−12	−2	−2	−5	−5	−8	−8	−10	−5·5	−12	−8
−90......	−0·5	−0·5	−3	−3	−5	−5	−1	−1	−2	−2	−3	−3	−4	−2·5	−5	−3
−100......	0	0	−1	−1	−2	−2	−0·5	−0·5	−1	−1	−1·5	−1·5	−2	−1	−2·5	−1·5

* The first columns under 7 and 8 being blank are omitted in printing.

Table IV.

Glass-hard-tempered Steel Wire.

Distances.	Deflections.								
	1.	2.	3.		4.	5.		6.	
100....	0·5	0	0·5	0	1	1·5	0	2	0
90....	1·5	0·5	1·5	0·5	2·5	3·5	0·5	4·5	0·5
80....	3	1·5	3	1	5·5	7	1·5	9	1·5
70....	9·5	4	9·5	2	17	22·5	3·5	27	3·5
68....	13	5·5	12	3	22	30	4	36	4
66....	18	7	15·5	3·5	28	39·5	4·5	47	4·5
64....	24	10	20·5	4·5	37	52·5	5·5	63	5·5
62....	31	13·5	25·5	5	49	71	6·5	82	6·5
60....	40	16	33·5	6·5	64	93	8	105	8
58....	49·5	21	41	8	79	115·5	10	131	10
56....	55	25	48	10	90	130	13	146	13
54....	56	25	51	12·5	96	136	17	152	16·5
52....	50	26	53·5	17·5	95	132	22	145	21
50....	40	26·5	55	24	91·5	122	29·5	135	28·5
48....	33	28	60	32	92·5	117·5	39	125	39
46....	25·5	30·5	68·5	42·5	99·5	120·5	52·5	126	50
44....	22	36·5	82	55·5	111	129	69	136	66
42....	24	43·5	97	70	125	142	85	148	84
40....	34	50	107·5	82	134	148·5	96·5	154	98·5
38....	52	53·5	108	85	132	142·5	98	146	102
36....	70	51	98·5	80	117	122	89·5	129	94·5
34....	80	45	80·5	67·5	94	97	74	106	79·5
32....	77	35	61·5	53	72	73·5	57	79	61
30....	67	27·5	45·5	39	54	53	42	59	45·5
28....	53	19·5	33	28	38·5	38·5	31	44	33·5
26....	41	14·5	23·5	21	28·5	28	22·5	33	24·5
24....	31·5	10	17	15	21	20·5	16	26	18
22....	25	7·5	13	11	16	18·5	12·5	20·5	14·5
20....	19	5	11	9	13	13·5	9·5	17·5	11
18....	15	4·5	9·5	7·5	11·5	11·5	9	15·5	9
16....	11	3·5	8	6	9·5	9·5	7	13	7·5
14....	8	3	7	5	8	8·5	6	11	6
12....	5·5	2·5	6	4	6·5	7·5	5	9·5	5
10....	4	2	5	3·5	5·5	6·5	4·5	8·5	4·5
8....	3·5	1·5	4·5	3	4·5	6	4	8	4
6....	3	1	4	2	3·5	5·5	3	8	3
4....	3	1	3	1·5	3	5	2	7	2
2....	3	0	2·5	1	2·5	4	1·5	5·5	1·5
0 ...	2	0	2	0·5	2	4	1	5	1

Table IV (continued).

Glass-hard-tempered Steel Wire.

Distances.	Deflections.								
	1.	2.	3.		4.	5.		6.	
− 2...	1	0	0·5	0	1	2	0	3	0
− 4...	2	0	0	− 0·5	0	1	− 0·5	1	− 0·5
− 6...	2	− 0·5	− 0·5	− 1	− 0·5	− 0·5	− 1	0	− 0·5
− 8...	2	− 1	− 1·5	− 1·5	− 1	− 1	− 1·5	− 1	− 1
−10...	1·5	− 1·5	− 2	− 2	− 2	− 2	− 2·5	− 1·5	− 2
−12...	1	− 2	− 2·5	− 2·5	− 3	− 3	− 3·5	− 2·5	− 2·5
−14...	0	− 2·5	− 4	− 3	− 4·5	− 4·5	− 4·5	− 4	− 3·5
−16...	− 1	− 2·5	− 5·5	− 4	− 6	− 6	− 5·5	− 6	− 5
−18...	− 2·5	− 3·5	− 7·5	− 6	− 9	− 9	− 7·5	− 9	− 7
−20...	− 4	− 5	−10	− 8	−12	−12·5	−10	−12	− 9
−22...	− 6	− 7·5	−14·5	−12	−17	−18	−14·5	−17	−13
−24...	− 8	−10·5	−19·5	−16·5	−23	−24	−19	−23	−17·5
−26...	−12·5	−15	−27	−23	−31·5	−33	−26	−33	−24
−28...	−17·5	−20	−35	−30	−40·5	−44	−34	−43·5	−32
−30...	−25	−27·5	−47·5	−40·5	−56	−59	−44	−59·5	−44
−32...	−34·5	−35·5	−63	−54	−76	−80·5	−59	−80	−58
−34...	−50·5	−45	−81	−68	−97	−104·5	−75	−105	−75
−36...	−66	−50	−98·5	−90	−118	−126	−91	−134	−93
−38...	−77·5	−53	−108	−85	−132·5	−145·5	−99	−154	−104
−40...	−81	−51·5	−107·5	−82	−135	−154	−97	−160	−102
−42...	−77	−44·5	−96·5	−70	−125	−145	−86·5	−153	−89·5
−44...	−70	−37	−82·5	−55·5	−110	−131	−68	−139	−70
−46...	−65	−31	−68	−42·5	−98	−117	−51	−132	−54
−48...	−61	−28·5	−59	−32	−90	−118	−37	−132	−40
−50...	−60	−26·5	−54·5	−24	−90·5	−124	−29·5	−141	−29
−52...	−63	−26	−53	−17·5	−92	−131	−22	−154	−22
−54...	−64	−25	−51	−12·5	−94	−134	−17	−160	−16·5
−56...	−60	−25	−48	−10	−89	−129	−13	−153	−12·5
−58...	−57	−21	−41	− 8	−80	−115	−10	−128	− 9·5
−60...	−45	−16	−33	− 6·5	−65	−92	− 8	−104	− 7·5
−62...	−33	−13	−25	− 5	−50	−70	− 6·5	−88	− 6·5
−64...	−23	−10	−20	− 4·5	−39	−52·5	− 5·5	−63	− 5·5
−66...	−17	− 7	−15	− 3·5	−29	−38·5	− 4·5	−47	− 4·5
−68...	−12	− 5·5	−12	− 3	−22	−30	− 4	−36	− 4
−70...	− 9·5	− 4	− 9·5	− 2	−17	−22·5	− 3·5	−27	− 3·5
−80...	− 3	− 1·5	− 3	− 1	− 5·5	− 7	− 1·5	− 9	− 1·5
−90...	− 1·5	− 0·5	− 1·5	− 0·5	− 2·5	− 3·5	− 0·5	− 4·5	− 0·5
−100...	− 0·5	0	− 0·5	0	− 1	− 1·5	0	− 2	0

Table V.—Cast-Iron Bar.

Distances.	Deflections.																
	1.	2.		3.	4.		5.	6.		7.	8.	9.	10.	11.	12.	13.	14.
100																	
90																	
80																	
70																	
60	8·5	33	17·5	73	88	46	96	105	50	..	..	..	..	51	35	− 26	− 88
58	9·5	37	20	83	100	52	110	120	56	..	..	..	..	58	40	− 30	−100
56	10·5	42	23	95	116	60	126	136	65	..	..	..	..	67	46	− 34	−116
54	12	47·5	26	109	133	70	144	157	75	..	..	..	..	78	53	− 38	−133
52	14	54·5	30	125	154	80	165	180	86	..	..	..	..	89	61	− 44	−154
50	16	63	34	143	176	92	190	208	99	..	..	..	..	102	71	− 51	−175
48	18·5	73	39	165	203	100	220	240	108	..	..	..	..	112	81	− 59	−202
46	21	84	46	191	233	122	251	276	130	..	..	..	..	136	93	− 67	−231
44	24·5	96	52	214	264	135	286	314	154	..	..	..	..	160	105	− 77	−262
42	28	109	60	248	303	161	328	355	171	..	..	..	..	178	121	− 88	−300
40	31·5	124	68	279	341	183	367	401	194	..	..	..	..	203	138	− 99	−336
38	35	138	77	310	380	205	410	446	216	..	..	..	..	226	154	−111	−375
36	38·5	154	86	343	418	228	450	490	240	..	..	..	..	251	171	−122	−412
34	41·5	169	95	374	453	250	488	529	263	..	..	..	..	274	188	−134	−447
32	44	183	105	400	484	269	519	630	282	..	..	..	..	294	203	−143	−478
30	47	193	114	419	503	285	541	580	310	..	..	..	..	313	215	−152	−495
28	49	202	120	430	512	298	550	590	322	600	690	742	760	323	224	−158	−508
26	49	207	124	432	510	304	546	584	328	..	..	..	..	328	229	−161	−505
24	49	205	127	424	498	300	529	573	318	..	..	..	..	325	227	−159	−494
22	47	201	125	405	470	292	500	531	308	..	..	..	..	314	222	−156	−468

Table V (continued).—Cast-Iron Bar.

Distances.	Deflections.																
	1.	2.		3.	4.		5.	6.		7.	8.	9.	10.	11.	12.	13.	14.
20	45	191	123	380	438	278	463	488	293	..	..	..	..	297	213	−148	−436
18	42	180	118	348	397	259	419	440	272	..	..	..	..	274	199	−140	−396
16	38·5	166	111	312	352	236	369	388	246	..	..	..	..	247	182	−128	−352
14	35	148	103	273	305	211	320	333	219	..	..	..	..	219	165	−116	−307
12	31	128	91	232	257	182	267	280	189	..	..	..	..	189	141	−102	−259
10	26	111	78	193	208	152	217	227	158	..	..	..	..	158	118	− 97	−215
8	22	87	63	151	165	122	170	177	124	..	..	..	..	124	88	− 72	−165
6	18	67	50	114	124	93	126	128	94	..	..	..	..	94	65	− 57	−125
4	13	42	34	74	79	61	83	85	62	..	..	..	..	62	43	− 42	− 79
2	7	21	16	36	37	30	40	42	30	..	..	..	..	30	19	− 20	− 39
0	2	1	0	0	0	0	0	0	1	..	..	..	..	1	0	0	0
− 2	− 7	− 20	− 16	− 36	− 37	− 30	..	..	− 30								
− 4	−13	− 40	− 34	− 74	− 79	− 61	..	..	− 62								
− 6	−18	− 64	− 50	−114	−124	− 93	..	..	− 94								
− 8	−22	− 86	− 63	−151	−165	−122	..	..	−124								
−10	−26	−107	− 78	−193	−208	−152	..	..	−158								
−12	−31	−126	− 91	−231	−257	−182	..	..	−189								
−14	−35	−146	−103	−272	−305	−210	..	..	−219								
−16	−38	−162	−111	−310	−352	−237	..	..	−246								
−18	−42·5	−178	−118	−348	−396	−259	..	..	−272								
−20	−45	−189	−123	−380	−436	−278	..	..	−293								

Table V (continued).—Cast-Iron Bar.

Distances.	Deflections.																	
	1.	2.		3.	4.		5.	6.		7.	8.	9.	10.	11.	12.	13.	14.
−22	−47·5	−198	−125	−404	−470	−292	..	..	−308								
−24	−50	−203	−127	−425	−495	−299	..	..	−318								
−26	−51	−206	−124	−422	−502	−304	..	..	−328								
−28	−51	−206	−120	−428	−510	−298	..	..	−323								
−30	−47	−196	−115	−418	−499	−286	..	..	−310								
−32	−44·5	−188	−106	−400	−482	−269	..	..	−285								
−34	−42	−170	− 95	−375	−452	−250	..	..	−264								
−36	−38·5	−156	− 86	−345	−417	−228	..	..	−241								
−38	−35	−140	− 77	−312	−380	−205	..	..	−216								
−40	−31·5	−138	− 68	−280	−340	−183	..	..	−196								
−42	−28	−110	− 60	−250	−302	−161	..	..	−172								
−44	−24·5	− 98	− 52	−215	−263	−142	..	..	−156								
−46	−21	− 86	− 46	−191	−232	−122	..	..	−132								
−48	−19	− 74	− 39	−165	−203	−106	..	..	−109								
−50	−16	− 63	− 34	−143	−176	− 92	..	..	− 99								
−52	−14	− 54	− 30	−125	−153	− 80	..	..	− 86								
−54	−12	− 48	− 26	−109	−132	− 70	..	..	− 75								
−56	−10·5	− 41	− 23	− 95	−115	− 60	..	..	− 65								
−58	− 9·5	− 36	− 20	− 83	−100	− 52	..	..	− 56								
−60	− 8·5	− 33	− 17·5	− 73	− 88	− 46	..	..	− 50								
−70																	
−80																	
−90																	
−100																	

Table VI.—Steel Bar, Hard-tempered.

Distances.	Deflections.								
	1.	2.	3.	4.	5.	6.	7.	8.	9.
100									
90									
80									
70									
60	108	...	...	...	103	90	6	− 62	− 70
58	123	...	...	...	117	104	8	− 71	− 78
56	141	...	...	...	135	120	10	− 80	− 90
54	160	...	...	...	156	138	12	− 91	−102
52	185	...	...	...	179	160	14	−102	−116
50	218	...	...	...	207	182	16	−118	−133
48	245	...	...	...	237	208	20	−133	−153
46	280	...	...	...	274	242	24	−153	−175
44	317	...	...	...	314	276	28	−173	−197
42	363	...	...	...	358	318	32	−201	−227
40	408	...	...	...	400	356	37	−210	−254
38	455	...	...	...	444	396	44	−243	−282
36	500	...	..	...	487	438	50	−266	−310
34	540	...	...	...	527	474	58	−291	−337
32	577	...	...	...	559	506	65	−313	−360
30	600	...	...	...	579	529	70	−326	−382
28	612	780	850	870	591	540	76	−336	−394
26	610	...	...	...	577	541	79	−343	−400
24	593	...	...	...	569	520	83	−340	−394
22	564	...	...	...	541	505	81	−334	−385
20	525	...	...	...	506	473	78	−320	−372
18	478	...	...	...	458	482	74	−300	−348
16	425	...	...	...	410	389	66	−278	−322
14	371	...	...	...	354	337	58	−256	−294
12	315	...	...	...	296	284	47	−226	−262
10	262	...	...	...	246	237	36	−196	−225
8	206	...	...	...	196	189	23	−166	−192
6	152	...	...	...	144	138	+10	−138	−157
4	98	...	...	...	90	83	− 5	−108	−122
2	48	...	...	...	43	39	−15	− 75	− 83
0	0	...	...	...	0	0	−30	− 42	− 44
− 2									
− 4									
− 6									
− 8									
− 10									
− 12									
− 14									
− 16									
− 18									
− 20									
− 22									
− 24									
− 26									
− 28									
− 30									
− 32									
− 34									
− 36									
− 38									
− 40									
− 42									
− 44									
− 46									
− 48									
− 50									
− 52									
− 54									
− 56									
− 58									
− 60									
− 70									
− 80									
− 90									
−100									

Table VII.—Malleable Iron Bar.

Distances.	Deflections.							
	1.	2.	3.	4.	5.	6.	7.	8.
100								
90								
80								
70								
60	42	102	143	151	182	191	16	− 32
58	48	116	163	172	208	217	18	− 37
56	54	133	186	198	238	251	21	− 42
54	62	152	212	225	274	287	24	− 48
52	71	174	244	260	315	320	28	− 55
50	81	201	280	290	362	370	33	− 62
48	93	230	323	342	416	424	38	− 72
46	107	265	370	395	479	498	43	− 82
44	123	303	424	451	545	570	50	− 94
42	140	347	485	514	622	649	57	−106
40	158	393	545	581	700	727	65	−120
38	177	440	608	648	781	808	73	−134
36	197	488	675	717	858	888	82	−149
34	216	536	739	781	928	959	90	−164
32	234	578	796	840	988	1020	98	−176
30	249	618	845	887	1024	1062	103	−189
28	260	644	870	916	1055	1080	112	−206
26	266	660	898	932	1058	1080	112	−204
24	267	663	897	926	1035	1051	111	−203
22	263	652	861	898	987	997	110	−202
20	254	629	823	856	923	928	107	−195
18	240	596	771	799	841	847	100	−185
16	220	543	708	728	751	749	94	−172
14	199	494	636	652	657	653	85	−154
12	176	434	556	560	560	552	75	−137
10	150	371	469	475	460	457	64	−115
8	121	301	378	382	364	364	53	− 95
6	93	231	285	288	262	262	40	− 74
4	63	155	182	195	169	168	27	− 52
2	31	75	92	94	86	85	13	− 25
0	0	0	0	0	0	0	0	0
− 2	− 31							
− 4	− 62							
− 6	− 92							
− 8	−120							
− 10	−149							
− 12	−176							
− 14	−198							
− 16	−218							
− 18	−238							
− 20	−252							
− 22	−261							
− 24	−265							
− 26	−265							
− 28	−259							
− 30	−248							
− 32	−234							
− 34	−216							
− 36	−197							
− 38	−178							
− 40	−158							
− 42	−140							
− 44	−125							
− 46	−109							
− 48	− 94							
− 50	− 81							
− 52	− 71							
− 54	− 62							
− 56	− 54							
− 58	− 48							
− 60	− 42							
− 70								
− 80								
− 90								
−100								

Let—

$(\alpha+\alpha')$=the area contained by the curve 1, the axis OY or OY', and the line YW or the line Y'W'.

α'=the area contained by the curve 2, the axis OY or OY', and the line YV or the line Y'V'.

l=half the length of the wire or bar.

l'= half the length of the coil.

r=the distance of the middle line of the wire or bar from the magnetometer needle.

m=the sum of all the magnetic matter, northern or southern, on either side of the centre of the wire or bar.

m'=the strength of the solenoid or coil.

S=the strength of the field at the point where the magnetometer needle hangs.

θ=the angle of deflection of the needle, in radian measure, corresponding to the division of the scale.

I=the intensity of the magnetisation of the wire at any cross-section, or intensity of magnetisation of the bar at its centre.

F=the magnetising force.

μ=the magnetising susceptibility.

a=the area of the cross-section of the wire or bar.

Then it can easily be proved, provided that the angles of deflections are so small as to be proportional to their tangents, as in the case we are considering, that $2\pi r \,.\, \mathrm{S} \,.\, \theta \,.\, \alpha$ is the integral sum of all the normal components of forces over the whole surface of a cylinder whose height is the length of the wire or bar, and whose radius is r, due to the magnetic matter m, situated at the centre of the cylinder, provided the length of the wire or bar be infinitely great; the correction for this length being $2l$ instead of infinite, is such that $3m\int_0^r \frac{2\pi r \,.\, l}{(l^2+r^2)^{\frac{3}{2}}}dr$ must be added to the above quantity to get the integral in question, neglecting, however, the sum of all the normal components due to $-m$, situated in the axis of the cylinder at a distance $2l$ from its centre, over that end of the cylinder which is farthest from $-m$. But the integral of the normal force N over any closed surface due to magnetic matter m inside is,

$$\int \mathrm{N}\, ds = 4\pi m;$$

hence $$2\pi r \,.\, \mathrm{S} \,.\, \theta \,.\, \alpha + 3m\int_0^{r} \frac{2\pi r \,.\, l}{(l^2+r^2)^{\frac{3}{2}}}dr = 4\pi m;$$

and hence $$2\pi r \,.\, \mathrm{S} \,.\, \theta \,.\, \alpha = 4\pi m\left\{\frac{3}{2}\frac{l}{(l^2+r^2)^{\frac{1}{2}}} - \frac{1}{2}\right\}.$$

Let now $$\frac{r}{2} \,.\, \mathrm{S} \,.\, \theta = \mathrm{R}, \text{ and } \left(\frac{3}{2}\frac{l}{\sqrt{(l^2+r^2)}} - \frac{1}{2}\right) = \mathrm{P},$$

then $$\mathrm{R}\alpha = \mathrm{P}m \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1).$$

Similarly,

$$2\pi r \,.\, \mathrm{S} \,.\, \theta\alpha' = 4\pi m' \left\{ \frac{l'}{\sqrt{(l'^2+r^2)}} - \tfrac{1}{2}\left(1 - \frac{2l-l'}{\sqrt{[(2l-l')^2+r^2]}}\right) \right\},$$

and hence $\qquad \mathrm{R}\alpha' = \mathrm{Q}m'$, say . . . . (2),

therefore $\qquad \mathrm{R}(\alpha+\alpha') = \mathrm{P}m + \mathrm{Q}m'$,

or, $\qquad m = \frac{\mathrm{R}}{\mathrm{P}}(\alpha+\alpha') - \frac{\mathrm{Q}}{\mathrm{P}}m'$ . . . . (3).

Also, $\qquad \mathrm{I} = \frac{m}{a}$ . . . . (4),

and hence, in the case of thin wires,

$$\mu = \frac{m}{a \,.\, \mathrm{F}}^{*} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (5).$$

The equation (2) gives us a means of ascertaining the value of m', if we know that of α', as in the case of 7 or 8, Table I. In the case where α' was not directly obtained by observation, m' was calculated from the following formula,

$$m' = c \times \mathrm{A} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6),$$

where A is the area contained by all the turns of coil per unit length, and c is the current strength in the coil. In the case of a cylindrical coil,

$$\mathrm{A} = n \,.\, \pi k^2 + n\pi\left(\frac{2b^2 \,.\, \frac{p-1}{2} \,.\, \frac{p+1}{2} \,.\, p}{2 \,.\, 3 \,.\, p}\right) = n\pi\left(k^2 + \frac{b^2(p-1)(p+1)}{12}\right);$$

in which n is the number of turns of wire per unit length of the coil, k the mean radius of the coil, p the number of layers, and b the mean distance between any two adjacent layers.

As to the evaluation of the magnetising force F. Let k be the mean radius of the cylindrical coil, or what is equivalent to it if the coil be not cylindrical; then the magnetising force at a point in the axis of the coil at a distance d from the centre, l', c, and n retaining the same signification as before, is,

$$\mathrm{F} = 2\pi nc\left\{ \frac{l'+d}{\sqrt{[k^2+(l'+d)^2]}} + \frac{l'-d}{\sqrt{[k^2+(l'-d)^2]}} \right\} \quad . \; . \quad (7).$$

At the centre of the coil, if l' be very great compared with k,

$$\mathrm{F} = 4\pi nc \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8).$$

Now it will be observed, as the equation (7) will show, that in my

* Papers on "Electricity and Magnetism," Sir William Thomson, p. 472; or Maxwell's "Electricity and Magnetism," vol. ii, p 68.

experiments the value of k was so very small and the magnetising force at any point of the wire or bar was so very slightly different from that at the centre, that the error which would arise from using the equation (8) will be very insignificant, and consequently this approximate equation was always used to evaluate F.

The current strength c was always measured on a Thomson tangent galvanometer, G, except when it was so weak that a small error in the galvanometer reading will produce a considerable error in the result, in which case the current was estimated from the electromotive force of the battery and the resistance of this circuit.

The strength, S, of the field was calculated in terms of H, the horizontal component of the terrestrial magnetism, simply by comparing the deflections of the magnetometer needle acted upon by a magnet (placed behind and at a convenient distance from the needle, and with its length in the line at right angles to the plane of the magnetic meridian) in the two cases: (1) When the field was due to the horizontal component H alone; and (2), when it was due to both the controlling magnet N and the horizontal force H. Since evidently the value of H seriously affects the results, it was thought desirable to make a fresh experiment to determine H at the very spot where the magnetometer needle is suspended. This was effected indirectly by counting the periods of a magnetic needle at the point in question, and at another point where the exact value of H was known from a direct experimental determination made after the manner described in my paper on "The Number of Electrostatic Units in the Electromagnetic Unit" ("Phil. Mag." for December, 1880), or more fully explained in Mr. Thomas Gray's paper on "The Experimental Determination of Magnetic Moments in Absolute Measure" ("Phil. Mag." for November, 1878); the value of H at the point where the magnetometer hangs was found to be ·1590. The value of V, the earth's vertical force, is of by far the less moment, considering that the only results the accuracy of which depends greatly upon that of the value of V, are those for μ for that particular magnetising force only; so that it was deemed unnecessary to find V by a new experiment, and consequently it was deduced from the value of H and that of the dip, 73° 45′ being taken for the latter according to the determination made some three years ago.

The final results tabulated at the end of the paper, namely, in the Tables A, B, C, D, &c., were derived from the mathematical considerations above discussed, and from the results given in the corresponding Tables of Deflection I, II, III, IV, &c., with the exception of the results given in 7, 8, 9, and 10 of the Table E, and 2, 3, and 4 of the Table F. The intensity, I, in these exceptional cases was obtained by assuming that the deflections of the magnetometer needle due to the magnetism of the bar alone (that is to say, the

deflections due to the coil being taken into consideration), corresponding to the distance 28 centims. (a distance approximately corresponding to a maximum deflection for high magnetising forces) are proportional to the intensity I. The deflections due to the coil alone were calculated from the strength of the current in the coil after a manner to be discussed later on.

Just a few words are perhaps necessary to explain the details of the Tables A, B, C, &c. In the first place the results given in the first, second, third, &c., horizontal lines along with the numbers 1, 2, 3, &c., in the Tables A, B, C, &c., correspond to the first, second, third, &c., vertical columns under the headings 1, 2, 3, &c., in the corresponding Tables of Deflection I, II, III, &c. No sign or a negative one is prefixed to the numbers, according as the polarities of the wire or bar or coil were similar or dissimilar to those induced in the wire by the earth's vertical force alone, if the numbers refer to the quantities indicating the magnetisation; or according as the magnetising forces were in a similar or dissimilar direction to the vertical force, if the numbers refer to the quantities representing the magnetising forces either directly or indirectly. Again, it will be observed that in 7 and 8 of the Table A, and of the Table C, there were obtained two values for $\frac{Q}{P}$, one calculated and the other observed; the object of this was two-fold: (1) To insure that the calculated value was within the errors of observations in the measurements of the current strengths, the dimensions of the coil, &c.; and (2), to render the results for this maximum magnetisation of the wires corresponding to these tables independent of the accuracy or inaccuracy of the measurement of the current strengths; the observed value for $\frac{Q}{P}m'$ was used, in these cases, to evaluate the quantities I, μ, &c.

The rest of what is given in the Tables A, B, C, &c., will, I hope, explain itself. But by far the readiest mode of studying the whole results, is to refer to the graphical representation shown in the Plates 11, 12, 13, 14, 15, the first three and the fourth of which contain the curves representing the "intensity of magnetisation" and the "magnetic susceptibility" respectively of the wires, and the last contains the curves representing the "intensity of magnetisation" of the bars. In other words, the curves in the Plates 11, 12, 13, and 15 are so drawn that the abscissæ are proportional to the magnetising force F, and the ordinates to the intensity I; whereas in the curves in the Plate 14 the abscissæ and the ordinates are proportional to the force F and the susceptibility μ respectively.

As regards, first of all, the Plates 11, 12, 13. The curves in the Diagram I correspond to the "Dark Wire," those in the Diagram II to the "Bright Wire," and those in the Diagram III to the "Steel Pianoforte

Wire" and "Glass-hard Steel Wire." Referring to the Diagram I, the curves (*a*) and (*b*) are those corresponding to the cases "On" and "Off" respectively directly after operating "Ons and Offs" while the magnetising force was acting; the curve (*c*) is one showing the effect of suddenly reversing the current in the coil; the curves (*d*) and (*e*) are those showing the effect of "Ons and Offs" while the reversed current was circulating through the coil, the former corresponding to the case "On" and the latter to "Off"; while the curve (*f*) is one so drawn that the ordinate at every point of it is half the algebraical difference of the ordinates of the curves (*b*) and (*e*), and hence exhibits approximately a curve which should have been obtained had the wire been experimented on without being subjected to the action of a pull. Had it not been for the sake of convenience of comparison, therefore, the curves (*c*), (*d*), and (*e*) should have been drawn on the negative side of the origin. Exactly the same explanation applies to the curves in the Diagram II as to the corresponding curves in the Diagram I.

In the Diagram III, the curves (*a*) and (*b*) show the results for "Steel Pianoforte Wire," and are subject to the same explanation as the corresponding curves (*a*) and (*b*) in the Diagram I or II; while the curves (*c*) and (*d*) refer to the "Glass-hard-tempered Wire" the former representing the result obtained when the magnetising force was in action, and the latter that obtained immediately after it was withdrawn.

Glancing at the curves in the Diagram I, we see something very striking. In the first place, we cannot help being struck with the remarkable effect of "Ons and Offs" on the magnetisation of the dark wire, when we compare the curve (*a*) or (*b*) with the curve (*f*). But a still more remarkable result is revealed in the fact that there is a surprising difference, as the curves (*a*) and (*b*) show, between the intensity of magnetisation of this wire in the case of "On," and that in the case of "Off" for low magnetising forces; and that the difference gets less and less remarkable as the magnetising force is more and more increased, becoming nothing at 15 units of the force, then changing into a negative quantity for still higher magnetising forces, and ultimately attaining a constant negative value. In other words, the intensity of magnetisation of the wire is greater or less while it is actually under the action of a constant pull than while it is free from it, according as the magnetising force to which the wire is subjected is below or above a certain value—a value which might, therefore, be called *critical*.* The fact that the two pairs of curves (*d*) and (*e*)

* This confirms the result given on page 62 of Sir William Thomson's paper on the "Electrodynamic Qualities of Metals, Part VII" ("Phil. Trans.," 1879), in which he calls this value "Villari Critical Value," as having been previously obtained by Villari.

and (*a*) and (*b*) are symmetrically placed with respect to the horizontal axis, each to each, shows that the ultimate effect of "Ons and Offs" is to magnetise the wire to the same degree of intensity, under the same circumstances, whether the magnetising force be in one or in the opposite direction. On the other hand, the curve (*c*) shows that when the magnetising force is so high as 60 units or so the wire seems to lose its retentiveness, so much so, that the reversal of the polarities of the wire by the reversal of the force is so complete that the operation of "Ons and Offs" produced no permanent effect; but that when the magnetising force is below that value the simple reversal of the force is not so effective as to annul the permanent effects of "Ons and Offs," or even to reverse the polarities of the wire. It is obvious that the excess of the intensity of magnetisation represented by the curve (*b*) over that represented by the curve (*f*), corresponding to any magnetising force, is a measure of the retentiveness of the wire for that magnetising force.

Remarks so very similar to those made on the curves in the Diagram I apply to the corresponding curves in the Diagram II that it is quite unnecessary to mention them. The comparison of the two sets of curves in the two diagrams, however, presents many points of interest. The curves (*a*) and (*b*) in these diagrams show that for some low magnetising forces the intensity of magnetisation of the "Bright Wire" is greater than that of the "Dark Wire;" this is, perhaps, not because the former is more susceptible of magnetisation than the latter, but chiefly because of the fact that there is for each wire a certain amount of pull (used for "Ons and Offs") which would give a maximum effect on the magnetisation of the wire, and that a weight of 12 kilogs. is nearer that value for the bright wire than a weight of 8 kilogs. is for the dark wire. As regards the critical point, we see that it is about 15 units in the case of the dark wire, while it is about 10 units in the case of the bright wire; but this point is no doubt different, not only for different kinds of wire but also for different amounts of the pull. But it is in the curve (*c*) that the chief interest lies. The comparison of the curves (*c*) and (*e*) in the two diagrams shows that the effect of reversing the magnetising force on the change or reversal of magnetisation is considerably less in the case of the bright wire than in the case of the dark wire, both which must doubtless be accounted for by supposing that the one (tolerably soft iron) has a greater coercive force than the other (exceedingly soft iron), as might be expected.

The comparison of the curves in the Diagram III with those in the Diagram I or II is also interesting. The most striking point is that, unlike the case of soft iron wires, there is no such thing as critical point in the case of steel wire, as the curves (*a*) and (*b*) in the Diagram III point out; for every magnetising force the intensity of

magnetisation is greater in the case of "Off" than it is in the case of "On." Comparing the curves (*a*) and (*b*) in the Diagrams I, II, and III, we notice a vast difference for low magnetising forces between the intensity of magnetisation of the pianoforte wire and that of the soft iron wire; but seeing that when the magnetising force is so high as 30 units or so (when the permanent effect of "Ons and Offs" begins to be insignificant, that is, when retentiveness gets inconsiderable), the intensity of magnetisation of the steel wire is very much the same as that of the soft iron wires, I think it probable that the above difference is, in a great measure, due to the fact that a weight of 16 kilogs. (less than one-sixth of the breaking weight of the pianoforte wire) used for the operation of "Ons and Offs" is far too small to produce anything like full effect on the magnetisation of the steel wire, and that this difference can be greatly diminished by using a heavier weight (perhaps 40 or 50 kilogs.) to operate "Ons and Offs." The difference that exists between the intensity of magnetisation of the steel pianoforte wire and that of the glass-hard-tempered steel wire, corresponding to low magnetising forces, is greatly due to a similar cause; but observing that there subsists a considerable difference in the intensity of magnetisation of these two wires even for so high a magnetising force as 50 or 60 units, it seems probable that the intensity of magnetisation of the glass-hard-tempered steel wire is really smaller for every magnetising force than that of the iron-tempered steel wire, even when the effect of stress is taken into account.

As regards the limit of the magnetisation of these wires, on comparing the curves (*a*) and (*b*) in these diagrams, it will be seen that that limit is attained at so low magnetising force as 80 units or so, both in the case of the soft iron wires and the non-tempered steel wire, and that the maximum magnetisation of the pianoforte wire is not lower than that of the soft iron wires in the ordinary cases—results certainly unexpected. On the other hand, the comparison of the curves (*b*) and (*c*) in the Diagram III requires a careful study. It shows that at about 80 or even 100 units of the magnetising force there is a notable difference between the magnetisation of the non-tempered and glass-hard-tempered steel wires; but whether this difference is due to the fact that the maximum magnetisation of the latter is not yet reached at the above-stated magnetising force, or it represents the actual difference in the maximum magnetisation of the two wires, it is difficult to decide. In whichever way this difference is accounted for, it is not unfair to say that the maximum magnetisation of the glass-hard-tempered steel wire is very nearly, if indeed not exactly, equal to that of the steel pianoforte wire or the soft iron wires, and that the minimum magnetising force corresponding to the maximum magnetisation is somewhat higher in the case of the former

than in the case of the latter. The values obtained of the maximum magnetisation of these wires are as follows:—

1. The dark wire..........	I = 1,390 / 1,420	corresponding to	"On." / "Off."
2. The bright wire.........	I = 1,360 / 1,415	"	"On." / "Off."
3. The steel pianoforte wire..	I = 1,370 / 1,420	"	"On." / "Off."
4. The glass-hard-tempered wire..............	I = 1,400 or 1,420?	"	"Off."

The curve (*a*) in the Diagram III shows that the maximum residual magnetism of the tempered steel wire is considerably greater than three-fourths of the total magnetism of which it is a residue; whereas in the case of the soft iron wires the maximum residual magnetism is only a small fraction of the total magnetism.

Passing now on to the curves in the Plate 14, no more words are perhaps necessary to explain them, because the explanations given of the curves in the Plates 11, 12, 13 will exactly apply to the corresponding curves in the Plate 14, if we substitute the words "Magnetic Susceptibility" for "Intensity of Magnetisation." By the corresponding curves is meant the curves which are marked by the same letters, such as (*a*), (*b*), &c., in the diagrams designated by the same numbers, such as I, II, &c.

With regard to the results for the magnetic susceptibility, it may be remarked that the results of the preliminary experiments not given in the paper, showed that the susceptibility of any one of the wires is different according to different circumstances under which it is placed, that is to say, that there is, for each magnetising force, an infinite number of values for the susceptibility corresponding to an infinite number of amounts of pull to the applications and removals of which the wire might have been subjected (though this appears to cease to be the case when the magnetising force exceeds a certain value, that is, when the wire begins to lose its retentiveness), not to speak at all of the different values for the susceptibility the wire has at any given stage of its history, according to the different amounts of a permanent pull to which the wire may be subjected. Hence it is evident that we should have a precise knowledge of the history, past and present, of the body whose susceptibility we wish to determine; and this is the very reason why the experiments were made on the wires under definite circumstances. The two sets of the values for the susceptibility of each wire, one for the case "On," and the other for "Off," given in the corresponding table and represented by the curves, are, therefore, those corresponding to that particular circumstance under which the wire was experimented on. The magnetic susceptibility of

the soft iron wires when retentiveness is disregarded, can be calculated, if required, from the magnetisation represented by the curves (f), Plates 11, 12, 13.

The greatest value for the magnetic susceptibility I obtained of soft iron wire is about 730, the corresponding magnetising force being the Glasgow vertical force, and it is probably still greater for smaller magnetising forces; while the magnetic susceptibility of the same wire for so high a magnetising force as 100 units, is only about 13, and still smaller, no doubt, for higher magnetising forces. These results are truly surprising, and will dispel any doubt as to the old view that the value of μ is constant or nearly so for all or a certain range of the magnetising force.

I will now proceed to explain the curves in the Plate 15 which represent the results for the bars. The "direct curves" show the results obtained by commencing with a small magnetising force which was gradually increased until it is so high as to magnetise the bars very strongly, if not to saturation; while the "return curves" represent the results obtained by coming down from a high magnetising force to lower and lower magnetising forces, passing through the zero and going up gradually to a high magnetising force on the negative side of the zero. It may be mentioned that the reason why for the steel bar the direct curve was not obtained is because the bar, which was one of those originally intended to be used for Sir William Thomson's new Siphon Recorder, was previously magnetised strongly, and, therefore, the experiment on it was commenced by using a high magnetising force to start with; and that there is every reason to believe that the direct curve for the steel bar is something like that for the cast-iron bar.

On comparing the "direct curves" in the Plate 15, we see that the magnetisation of the cast-iron bar is somewhat less for high magnetising forces than that of the steel bar, and is much less for every magnetising force than that of the soft iron bar; and that the maximum magnetisation of the soft iron bar is about 1340, that of the steel bar is about 860, and that of the cast-iron bar is only about 770, while the corresponding least magnetising force in the case of the first is only about 190 units, and in the case of the second and third, it is roughly 450 and 400 units or more. Of course, it is not quite right to assume that the above results represent accurate comparisons of the magnetisable qualities of those different kinds of iron and steel, because the bars are not the same in dimensions, which have very considerable effects on the intensity of magnetisation. Still considering that the difference in dimensions between the soft iron bar and the other bars is very small, while in both the maximum intensity of magnetisation and the minimum magnetising force corresponding to it they differ greatly from each other, it is certain

that both the cast-iron bar and the steel bar are greatly inferior to the soft iron bar in respect to magnetisability. This is indeed unexpected, and in some measure astonishing, remembering that the steel pianoforte wire was not at all inferior in this respect to the soft iron wires, at least for higher magnetising forces. The difference that is found in the maximum intensity of magnetisation and the minimum magnetising force corresponding to that magnetisation between the soft iron bar and the wires is, however, no doubt, chiefly due to the effects of the dimensions of the bar.

Another point of interest lies in the "return curves."* They show that in the case of each bar the magnetisation of the bar did not reverse until the magnetising force exceeded a certain value on the negative side of the zero, and that this value is considerable even in the case of the soft iron bar, considerably greater in the case of the cast iron, and still greater—greater by a vast amount—in the case of the steel bar. A complete curve for the residual magnetism was only obtained, or at least only shown, for the cast iron; but the fact that those points in the return curves corresponding to the zero magnetising force represent the maximum residual magnetism of the corresponding bars, will give us a rough indication of what might be the residual magnetism curves for the other bars.

I have now given the general explanations and discussions of all the results of the experiments, and as I fear space does not permit me to enter into a fuller discussion of all the details of the results and of the inferences that can be drawn from them, I am obliged to leave them untouched. There is, however, one very interesting and important conclusion which can be derived from the results and which I cannot help noting specially, as it illustrates the beauty of this magnetometric method, and that is, in regard to the change in the distribution of magnetism of the wires or bars due to the corresponding change in the magnetising force to which they are subjected. It has already been said that one way to study the results given in the Tables I, II, &c., is to trace curves in the manner explained. Now, it is easy to get two such curves as (1) and (2) of the Plate 10 for each set of the results, one representing the effect due to both the magnetism of the wire or bar, and the coil carrying a current, and the other representing the same due to the coil alone. If we draw another curve such that its abscissa at every point of it is the difference of the abscissæ at the same point of the two curves, we obtain a curve representing the effect due to the magnetism of the wire or bar alone. The curve representing the effect of the coil alone can be easily

* Compare these curves with those given in Sir William Thomson's paper referred to before, "Phil. Trans.," 1879, Plates 8 and 9.

obtained, if necessary, from the value of m', because evidently the curve represented by the equation,

$$x = \frac{m' \cdot r}{S \cdot \theta} \cdot \left\{ \frac{1}{(r^2+(l'-y)^2)^{\frac{3}{2}}} - \frac{1}{(r^2+(l'+y)^2)^{\frac{3}{2}}} \right\} \quad . \quad . \quad (9),$$

in which r, S, θ, l', &c., retain the same meaning as before, will be the one required, namely, one in which the ordinates are proportional to the vertical distances of the magnetometer needle from the centre of the coil, and the abscissæ to the deflections of the needle due to the coil.

A theoretical curve representing the effect due to the magnetism of the wire or bar solenoidally distributed, that is to say, with a certain quantity of free magnetic matter of northern polarity at one extremity and the same quantity of free magnetic matter of southern polarity at the other extremity of the wire or bar, can be obtained in a similar way; in fact, the equation (9) will represent such a curve, if we substitute the quantity of the free magnetic matter at either end of the wire or bar for m' and half the length of the wire or bar for l'.

Now the curves (1), (2), (3), and (4), in the Plate 16, were obtained in the way just explained from the results given in 1, 2, 4, and 7, and the Tables I and II (that is, the results for the "Dark Wire"); they represent the curves showing the effects due to the magnetism of the wire alone, and correspond respectively to ·545 (in vertical force), 2·35, 14·08, and 80·7 units of the magnetising force, while (5) is a theoretical curve representing the effect which should have been obtained had the same wire been magnetised solenoidally, so as to contain 8 units of the quantity of free magnetic matter of one polarity at one end of it, and the same quantity of matter of opposite polarity at the other end. These curves form the true comparisons of the magnetisations of the wire in the different cases, because they are all reduced to the same standard, that is to say, they are all so drawn that their abscissæ represent the deflections of the magnetometer needle which should have been obtained had the field S been one and the same, namely, 1·873 units in all cases.

The comparisons of the curves (1), (2), (3), and (4) show that the greater the magnetising force the greater is the distance from the centre or origin of the points of the ordinates corresponding to the maximum deflections of the magnetometer needle, while the comparison of the curves (4) and (5) shows that these points in the case of the curve (4) are almost, if not exactly, coinciding with those in the case of (5); showing quite distinctly that the magnetisation of the wire for a low magnetising force is far from being solenoidal, but stronger at the central parts of the wire than in the other parts; but that as this force is made stronger and stronger, the magnetism of the wire becomes more and more equally distributed to the ends until the dis-

tribution becomes nearly, if not altogether, solenoidal, when the force is made so high as to give the wire the maximum magnetisation. More or less similar facts can be arrived at from the results for other wires, and also those for the bars.

These facts are truly interesting, seeing that they entirely agree with theoretical considerations. Indeed they have been pointed out theoretically by Sir William Thomson,* and indicated experimentally by Rowland.† But I believe my experiments are the first, the results of which have brought out those facts so clearly as not only to leave no room for doubt, but also to enable us to see the law by which the change in the distribution of magnetism in a cylindrical rod due to the change of magnetising force to which it is subjected, is governed; and I hope they will be of service in guiding the future investigators of electro-magnetism or otherwise.

It is impossible for me to conclude this paper without expressing my most grateful thanks to Sir William Thomson for the very kind guidance and instruction he has given me in the course of these experiments.

* Papers on "Electricity and Magnetism," § 667.

† "Phil. Mag.," August, 1873, p. 142.

Table A.—Dark Soft Iron Wire.

Number of heading under deflection in Table I.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$.	$\frac{Qm'}{P}$. Calculated.	$\frac{Qm'}{P}$. Observed.	m.	I.	F.	μ.
1........	H = 0·1590	8,340	0	1·704	0	..	1·704	400	V = 0·545	734
	,,	3,800	0	0·7766	0	..	0·7766	182	,,	335
2........	,,	11,740	0·00865	2·400	0·0522	..	2·348	551	1·806 + V = 2·35	235
	,,	7,780	,,	1·590	,,	..	1·538	361	,,	154
3........	,,	16,070	0·0229	3·283	0·138	..	3·145	738	5·33	139
	,,	12,320	,,	2·518	,,	..	2·380	559	,,	105
4........	H × 11·92 = 1·895	1,880	0·0[illegible]48	4·566	0·3914	..	4·175	980	14·08	69·6
	,,	1,830	,,	4·445	,,	..	4·054	952	,,	67·6
5........	,,	2,500	Mean = 0·1605	6·071	0·969	..	5·102	1266	34·07	35·2
	,,	2,620	,,	6·363	,,	..	5·394	1198	,,	37·2
6........	H × 11·78 = 1·873 ,,	3,930	0·0538	9·441	3·648	..	5·793	1360	56·7	24·0
7........	,,	4,600	Varied between	11·08	5·21	..	5·93	1390	80·7	17·1
	,,	4,670	,,	11·24	,,	..	6·09	1430	,,	17·6
	,,	2,180	,,	..	..	5·15				

Table A (continued).—Dark Soft Iron Wire.

Number of heading under deflection in Table I.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$.	$\frac{Q}{P}m'$. Calculated.	$\frac{Q}{P}m'$. Observed.	m.	I.	F.	μ.
8........	H × 11·78 = 1·873	5,270	.. ..	12·69	6·80	..	5·93	1390	105	13·2
	,,	5,330	Varied between	12·82	,,	..	6·05	1420	,,	13·5
	,,	2,810	,,	..	..	6·76				
12........	,,	−2,335	−0·0196	−5·622	−1·329	..	−4·293	−1010	−19·9	
	,,	−2,420	,,	−5·731	−1·329	..	−4·403	−1060	,,	
	,,	−2,510	,,	−6·043	,,	..	−4·714	−1110	,,	
13........	,,	−1,630	−0·0109	−3·924	−0·739	..	−3·185	− 748	−10·7	
	,,	−1,930	,,	−4·647	,,	..	−3·908	− 917	,,	
	,,	−1,870	,,	−4·502	,,	..	−3·763	− 833	,,	
14........	,,	−1,110	−0·00684	−2·673	−0·464	..	−2·209	− 518	−6·60	
	,,	−1,460	,,	−3·516	,,	..	−3·052	− 716	,,	
15........	H = 0·1590	−5,555	−0·00341	−1·135	−0·231	..	−0·904	− 212	−3·015	
	,,	−10,840	,,	−2·216	,,	..	−1·995	− 467	,,	
16........	,,	3,900	−0·000928	0·797	−0·0629	..	0·860	202	−0·424	
	,,	−3,770	,,	−0·7703	,,	..	−0·707	− 166	−0·424	
17........	,,	4,630	−0·000785	0·946	−0·0532	..	0·999	234	−0·275	
	,,	−1,660	,,	−0·339	,,	..	−0·286	− 67·1	,,	

Table B.—Bright Wire.

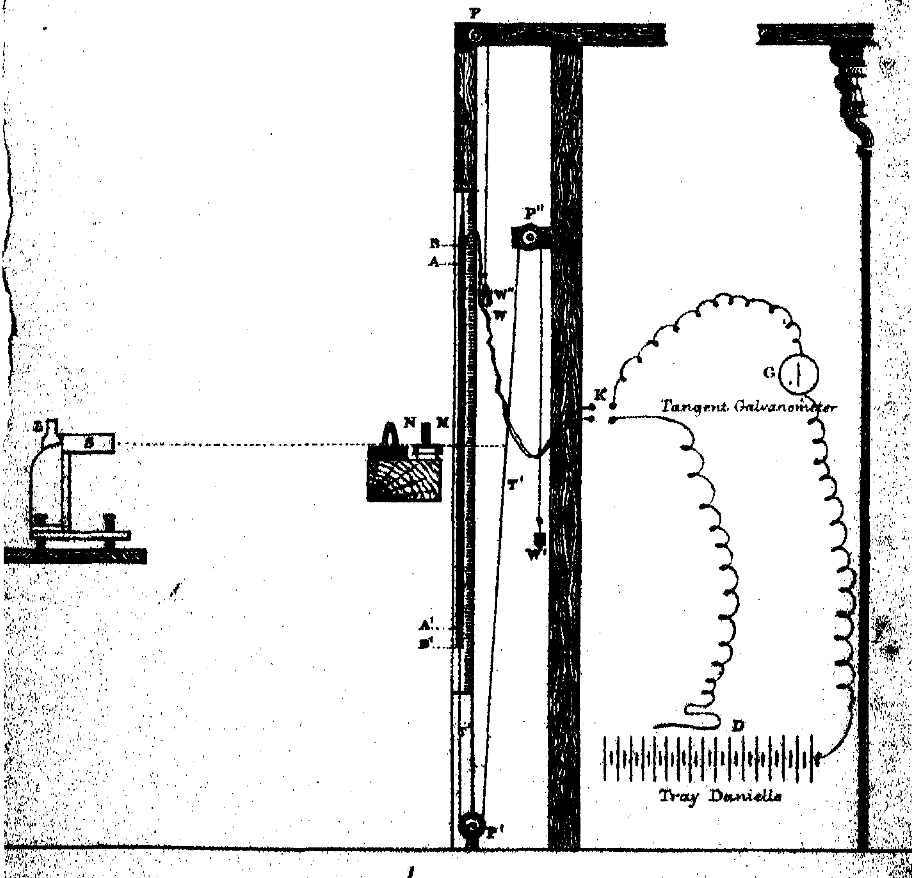
Number of heading under deflection in Table II.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$	$\frac{Q}{P}m'$. Calculated.	$\frac{Q}{P}m'$. Observed.	m.	I.	F.	μ.
1........	H = ·1590	9630	0	1·966	0	..	1·966	328	V = 0·545	602
	H = ·1590	6710	0	1·370	0	..	1·370	228	,,	419
	,,	5290	0	1·080	0	..	1·080	181	,,	
	,,	−5690	0	−1·366	0	..	−1·366	−227	,,	
2........	H × 11·7 =1·860	1470	0·001686	3·513	0·114	..	3·399	567	2·31	245
	,,	898	,,	2·146	,,	..	2·032	340	−1·21	
3........	,,	2410	0·00341	5·758	0·231	..	5·527	923	4·11	225
	,,	2040	0·00341	4·874	0·231	..	4·643	776	4·11	188
	,,	550	−0·00341	1·314	−0·231	..	1·543	251	−3·01	
	,,	−1840	−0·00341	−4·396	−0·231	..	−4·165	−696	−3·01	
4........	,,	2730	0·00702	6·524	0·476	..	6·048	1010	7·87	127
	,,	2620	0·00702	6·261	0·476	..	5·785	950	7·87	120
	,,	− 870	−0·00702	−2·079	−0·476	..	−1·603	−267	−6·78	
	,,	−2540	0·00702	−6·068	−0·476	..	−5·592	−931	−6·78	
5........	,,	3130	0·0141	7·478	1·008	..	6·470	1090	15·00	73·5
	,,	3350	,,	8·004	,,	..	6·996	1180	15·0	79
	,,	−2540	−0·0141	−6·068	−1·008	..	−5·060	−857	−13·8	
	,,	−3310	,,	−7·909	,,	..	−6·901	−1165	,,	

Table B (continued).—Bright Wire.

Number of heading under deflection in Table II.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha)$	$\frac{Q}{P}m'$. Calculated.	$\frac{Q}{P}m'$. Observed.	m.	I.	F.	μ.
6........	H × 11·7 =1·860	3790	Mean = ·0276	9·055	1·871	..	7·244	1210	29·4	41·1
	"	3980	"	9·510	"	..	7·639	1275	"	43·3
	"	−3800	Mean = − ·0276	−9·081	−1·871	..	−7·210	−1205	−28·3	
	"	−3920	"	−9·367	"	..	−7·496	−1220	−28·3	
7........	" "	5000	Varied between ·0558 and ·0552 Mean = ·0555	11·95	3·76	..	8·19	1370	58·4	23·3
	"	−4980	Mean = − ·0555	−11·90	−3·76	..	−8·14	−1360	−57·4	
	"	−5000	"	−11·95	"	..	−8·19	−1370	"	
8........	"	5540	Varied between ·0805 and ·0790 Mean = 0·0798	13·23	5·41	..	7·82	1320	83·9	15·7
	"	5790	"	13·84	5·41	..	8·43	1410	"	16·8
	"	−5540	Mean = − ·0798	−13·23	−5·41	..	−8·43	−1410	−82·8	
9........	"	6420	Varied between ·1025 and ·1005 Mean = ·1015	15·34	6·88	..	8·46	+1415	106·5	13·3

Table C.—Pianoforte Wire.

Number of heading under deflection in Table III.	S.	$x+x'$.	c.	$\frac{R}{P}(x+x')$.	$\frac{Q}{P}m'$. Calculated.	$\frac{Q}{P}m'$. Observed.	m.	I.	F.	μ.
1........	H = 0·1590	800	0	0·1637	0	..	0·1637	37·8	V = ·545	67·5
	"	820	0	0·1678	0	..	0·1678	38·6	"	69·3
2........	"	3570	0·00338	0·7305	0·229	..	0·5015	112·7	4·074	27·6
	"	3840	"	0·7856	"	..	0·5566	125	"	30·7
3........	"	7410	0·00704	1·517	0·477	..	1·040	233	7·89	29·6
	"	8070	"	1·651	"	..	1·174	258	"	33·4
4........	H × 11·7 = 1·860	1300	0·0142	3·113	0·963	..	2·150	484	14·8	31·6
	"	1340	"	3·209	"	..	2·246	504	"	33·0
5........	"	2960	0·0288	7·089	1·953	..	5·036	1132	30·6	37·0
	"	3130	"	7·496	"	..	5·543	1245	"	40·7
6........	"	3950	·0552	9·461	3·743	..	5·718	1284	58·2	22·1
	"	4110	"	9·842	"	..	6·099	1370	"	23·5
7........	"	4820	Varied between ·0783 & ·0773 Mean = ·0778	11·54	5·28	..	6·30	1415	81·7	17·3
	"	2190	"	..	..	5·24				
8........	"	5540	Varied between ·1036 & ·1014 Mean = ·1025	13·23	6·95	..	6·33	1420	107·5	13·2
	"	2880	"	..	..	6·95				



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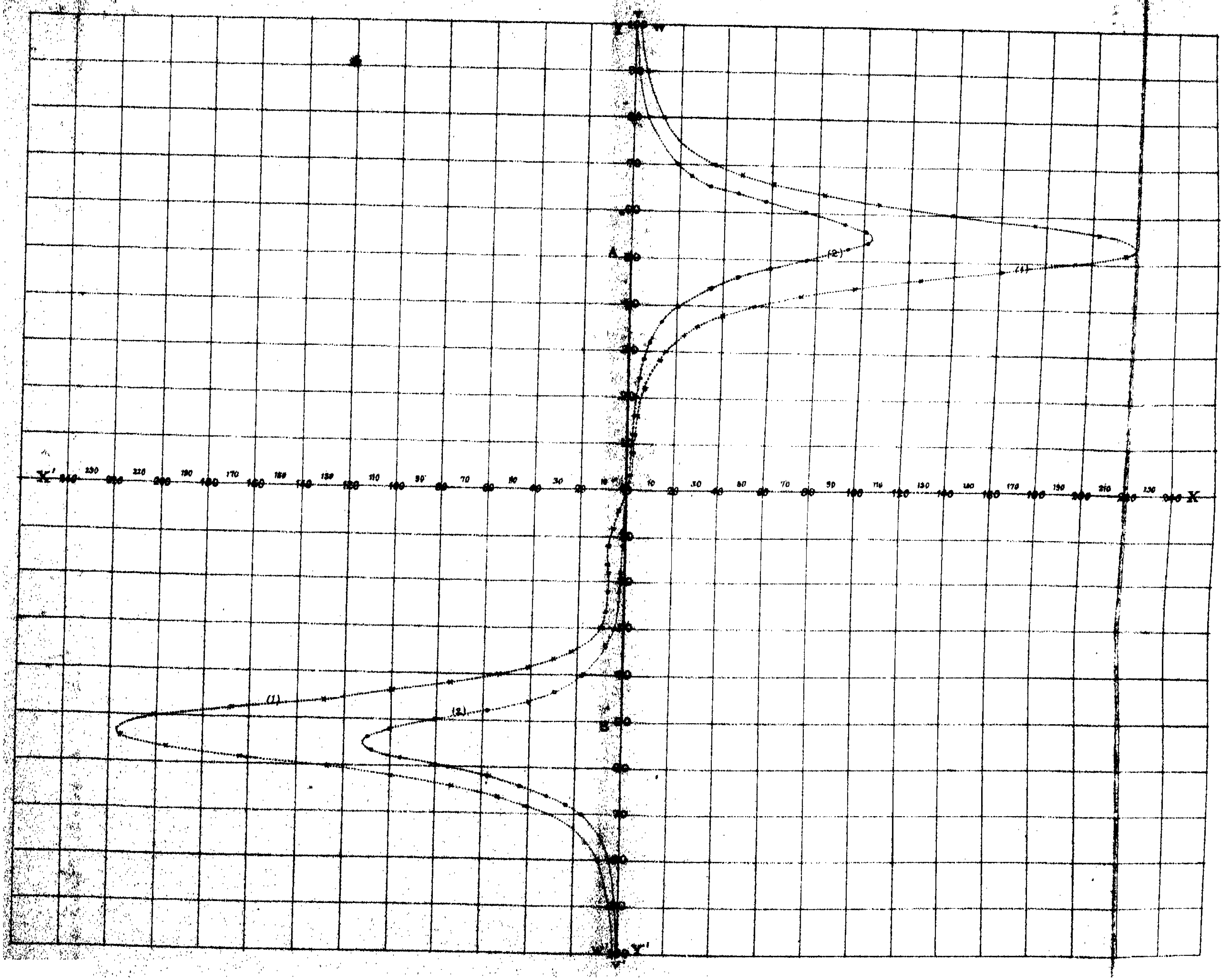
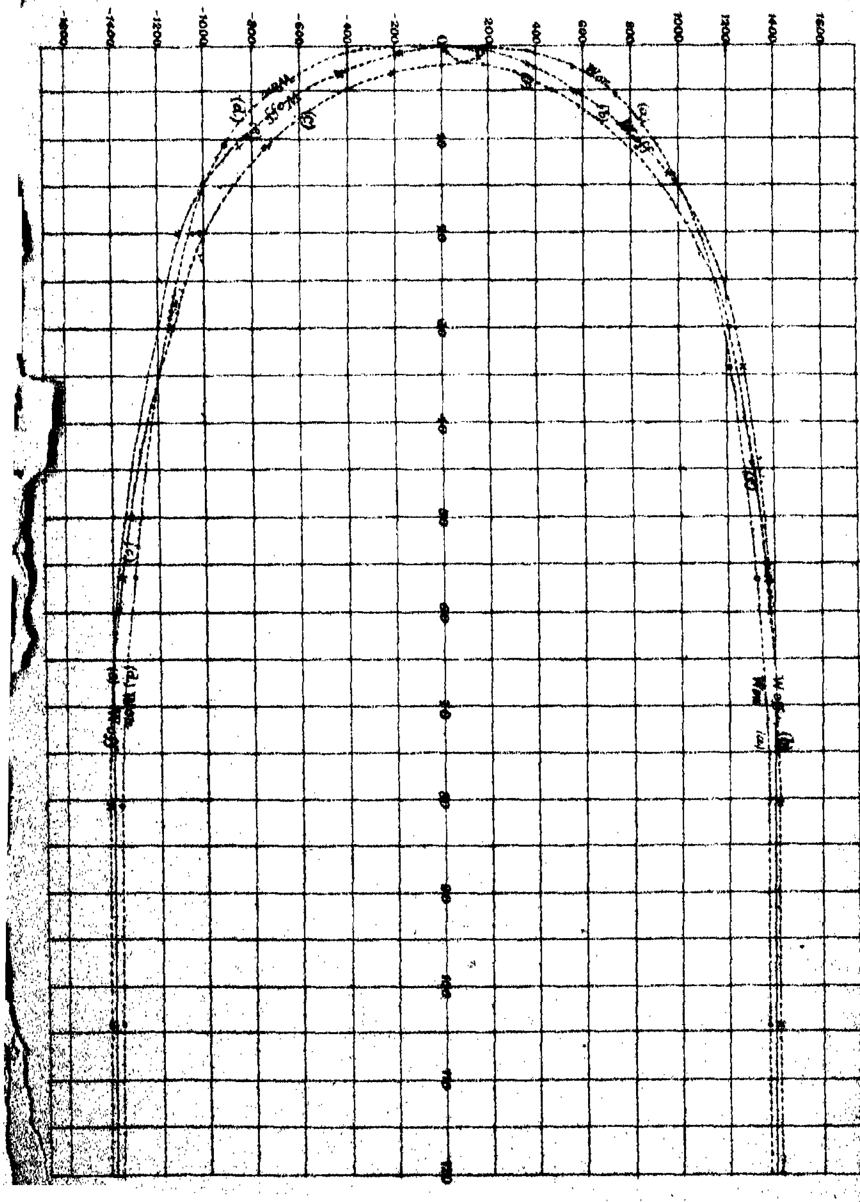
Proc. Roy. Soc. Vol. 35 Pl. 10.



Intensity of Magnetization - Curves.
DIAGRAM I. (Dark wire, - W = 8 kilogs.)



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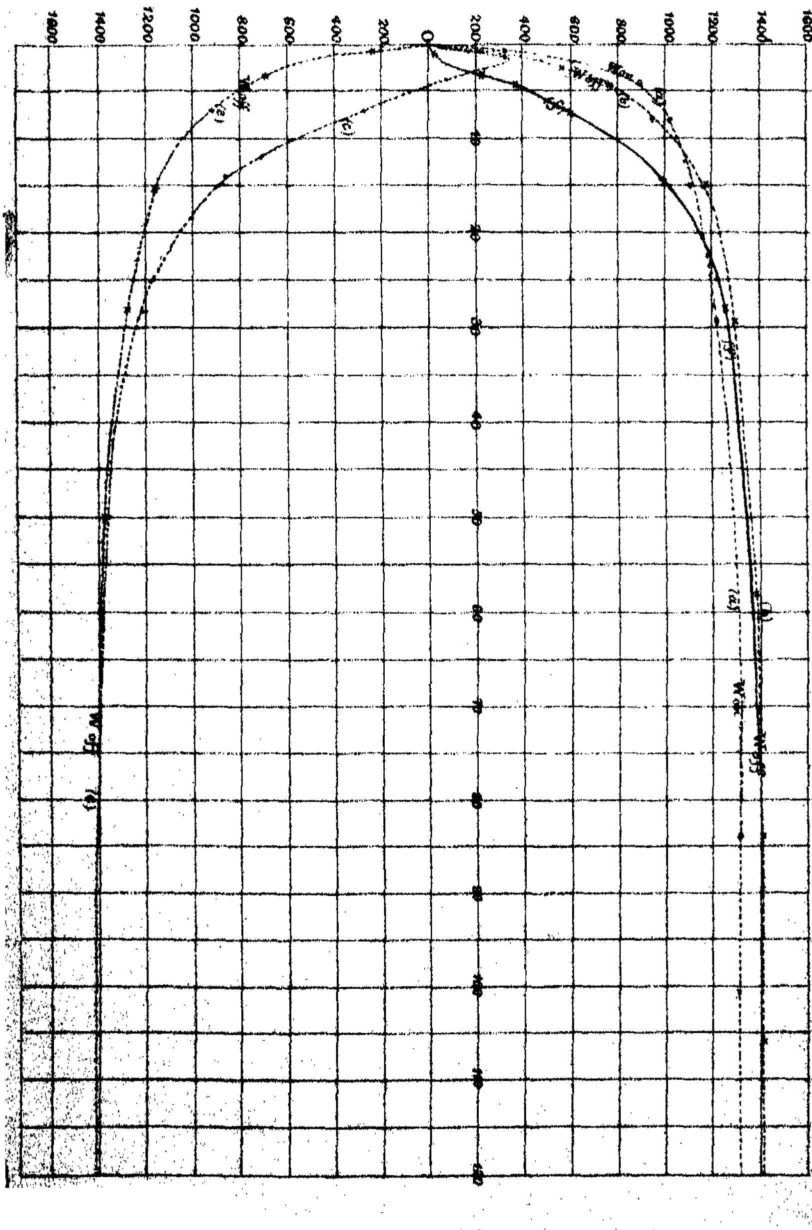
Intensity of Magnetization - Curves.

DIAGRAM II. (*Bright wire — W = 8 kilogs.*)



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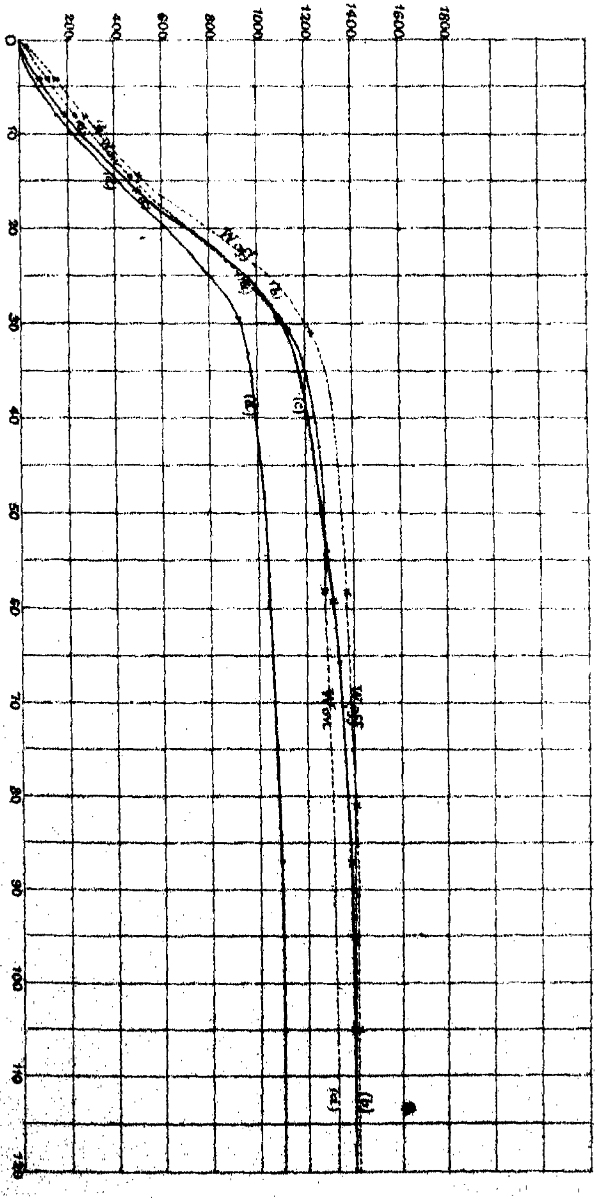
"Intensity of Magnetization - Curves.

DIAGRAM III. (Non-tempered and tempered Steel Pianoforte-wire) W = 8 kilogs.



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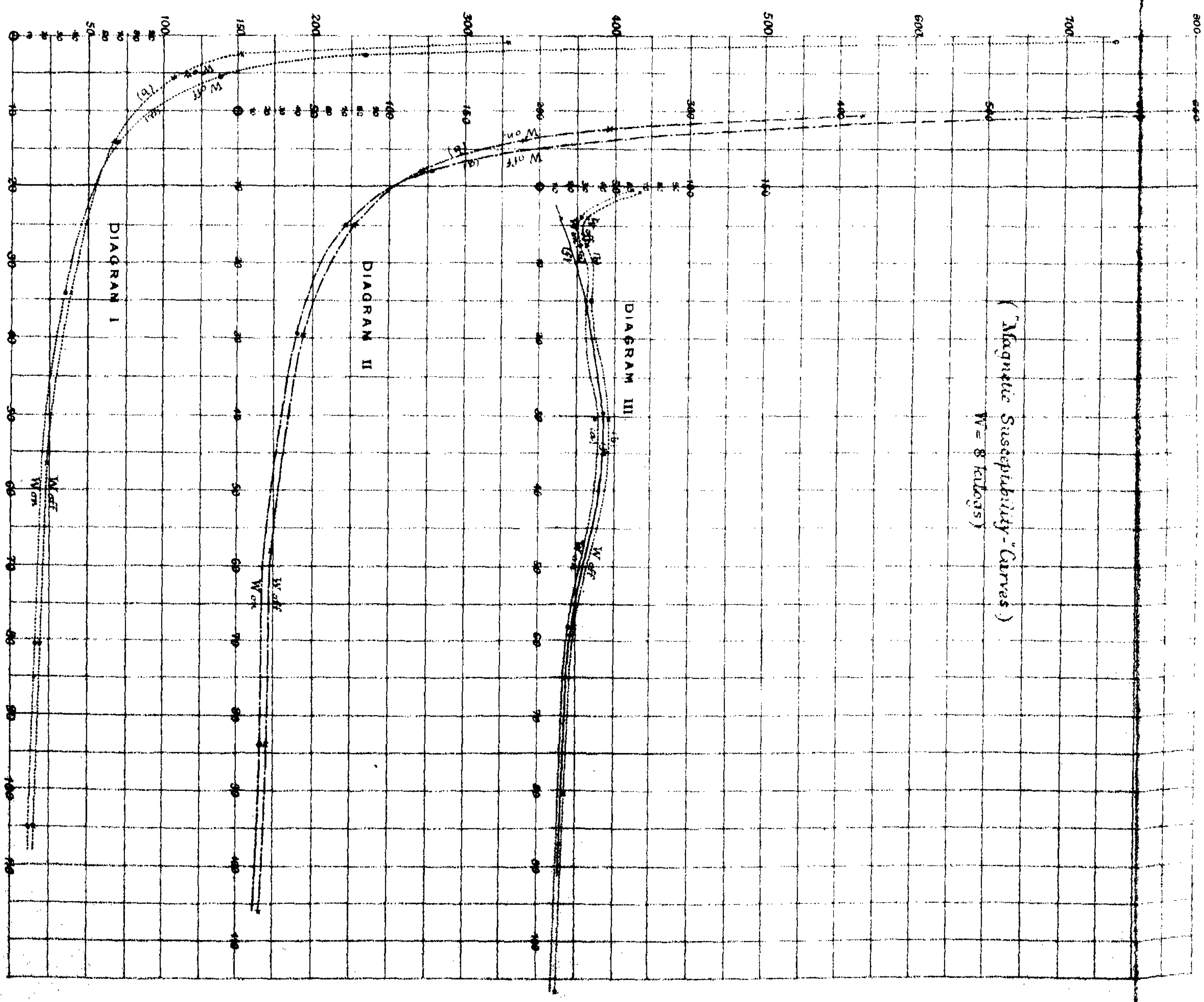

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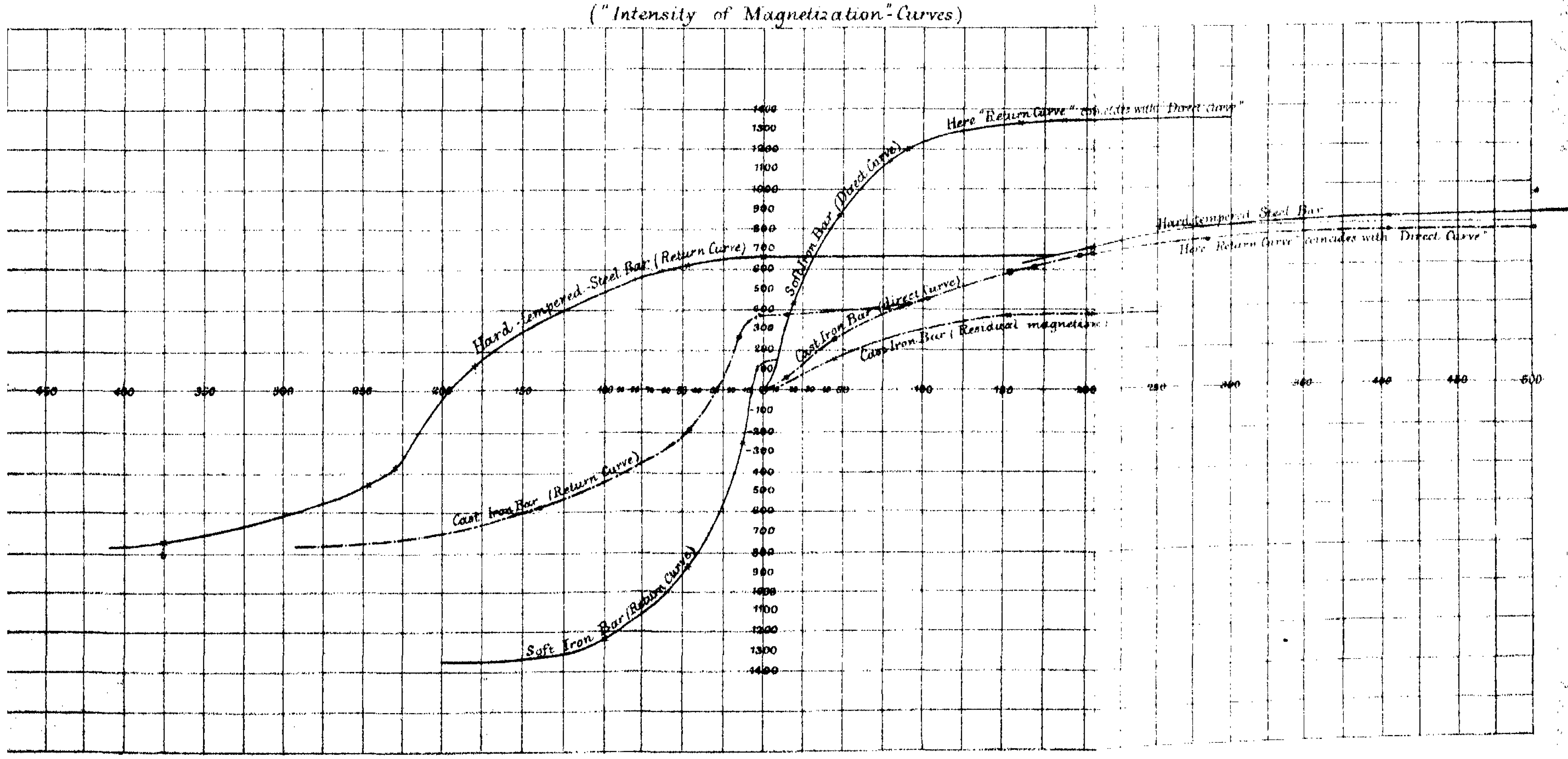
("Intensity of Magnetization" Curves)



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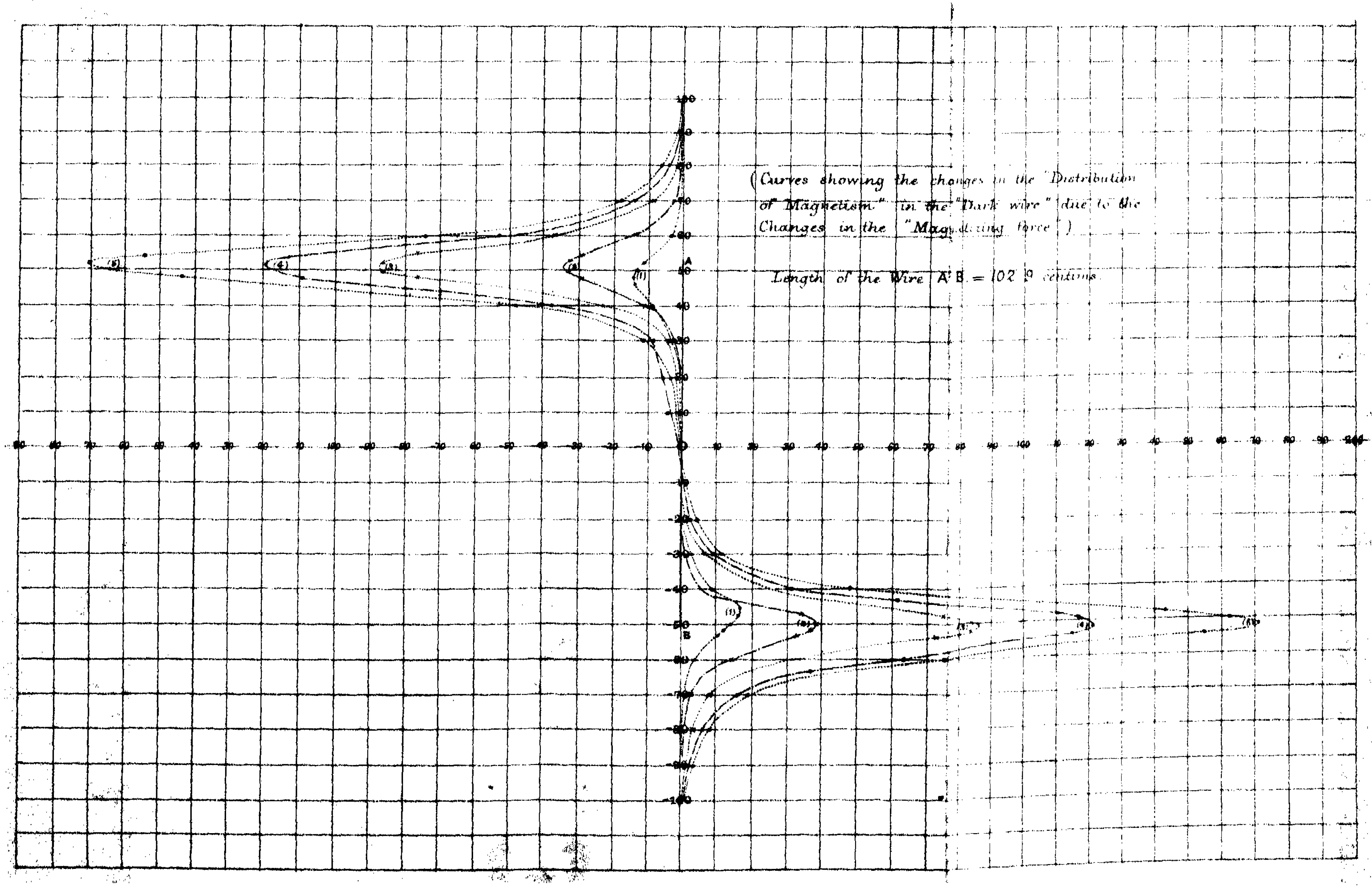


West, Newman & Co. lith.

Table D.—Glass-hard-tempered Wire.

Number of heading under deflection in Table IV.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$.	$\frac{Q}{P}m'$. Calculated.	$\frac{Q}{P}m'$. Observed.	m.	I.	F.	μ.
1.	H = 0·159	2300	0·00328	0·4796	0·2201	..	0·2595	59·4	3·97	15·0
2.	H × 11·7 = 1·860	1334	0·0149	3·245	0·980	..	2·265	519	15·8	32·8
3.	,, ,,	2730 1670	0·0276 0	6·662 4·075	1·852 0	.. ..	4·81 4·075	1100 930	29·4 0	37·5
4.	,,	3880	0·0563	9·491	3·778	..	5·713	1310	59·3	22·1
5.	,, ,,	4760 1960	Mean = ·0825 0	11·61 4·783	5·54 0	.. ..	6·07 4·783	1390 1090	86·7 0	16·0
6.	,, ,,	5290 1960	Mean = ·1015 0	12·91 4·783	6·81 0	.. ..	6·10 ..	1400 1090	106·4 0	13·3

Table E.—Cast-Iron Bar.

Number of heading under deflection in Table V.	S.	$a+a'$.	c.	$\frac{R}{P}(a+a)$.	$\frac{Q}{P}m'$.	m.	I.	F.
1	8·835	1,750	0·0424	59·33	3·62	55·71	58·6	14·6
2	,,	7,180	Varied from ·133 to ·131 Mean = ·132	243·4	11·3	232·1	244	45·4
	,,	4,320	0	147·5	0	147·5	155	,,
3	,,	13,560	Varied from ·312 to ·286 Mean = ·299	459·7	25·5	434·2	457	103
4	,,	17,380	Varied from ·470 to ·430 Mean = ·450	589·2	38·4	550·8	579·8	155
	,,	10,310	0	349·5	0	349·5	368	,,
5	,,	18,600	Varied from ·534 to ·462 Mean = ·498	630·6	42·5	588·1	619	171
6	,,	20,050	Varied from ·624 to ·524 Mean = ·574	679·8	49	630·8	664	197
	,,	10,940	,,	371	0	371	390	,,
7	,,	...	0·610	...	...	...	672	208
8	,,	...	0·920	...	...	...	731	316
9	,,	...	1·250	...	...	...	765	430
10	,,	...	1·460	...	...	...	767	502
11	,,	11,160	0·042	378·4	3·59	374·8	394·6	14·4
12	,,	7,820	−0·0392	265·2	− 3·37	268·6	283	− 13·6
13	,,	− 5,560	Varied from ·130 to ·128 Mean = − ·129	−188·6	−11	−177·6	−187	− 44·4
14	,,	−17,300	Varied from ·43 to ·39 Mean = − ·410	−586	−35	−551·5	−580	−141

Table F.—Steel Bar, Hard-tempered.

Number of heading under deflection in Table VI.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$.	$\frac{Q}{P}m'$.	m.	I.	F.
1.	8·835	20,750	Varied between ·608 and ·580 Mean = ·594	703·7	50·74	653	689	205
2.	„	..	1·18	..	..	..	815	406
3.	„	..	1·54	..	..	..	867	526
4.	„	..	1·82	..	..	..	867	626
5.	8·06	20,190	0	624·4	0	624·4	659	0
6.	„	18,470	−0·142	571·2	−12·13	583·3	613	−48·8
7.	„	2,320	Varied between ·537 and ·505 Mean = ·521	71·76	−44·5	116·3	123	−179
8.	„	−12,620	Varied between ·696 and ·632 Mean = ·664	−390·4	−56·7	−333·7	−352	−228
9.	„	−15,480	Varied between ·742 and ·680 Mean = ·711	−478·7	−60·7	−418	−441	−244

Table G.—Malleable Iron Bar.

Number of heading under deflection in Table VII.	S.	$\alpha+\alpha'$.	c.	$\frac{R}{P}(\alpha+\alpha')$	$\frac{Q}{P}m'$.	m.	I.	F.
1..........	8·835	9,260	·053	313·9	4·53	309·4	343	18·2
2..........	"	23,050	·139	781·4	11·9	769·5	854	47·8
3..........	"	30,930	Varied between ·238 and ·218 Mean = ·228	1049	19·5	1030	1143	78·4
4..........	"	32,340	Varied between ·266 and ·252 Mean = ·259	1096	22·1	1074	1192	89·0
5..........	"	36,640	Varied between ·490 and ·450 Mean = ·470	1243	40·1	1203	1336	163
6..........	"	37,030	Varied between ·594 and ·544 Mean = ·570	1255	48·7	1206	1339	189
7..........	"	−7,150	− ·053	−242·4	−4·5	−237·9	−264	− 18·2
8..........		−23,050	− ·139	−781·4	−11·9	−769·5	−854	−47·8

"On the Effect of Electrical Stimulation of the Frog's Heart, and its Modification by Heat, Cold, and the Action of Drugs." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received May 16, 1881. Read June 16, 1881. Revised June 13, 1883.

In the following research we have examined the effect of electrical stimuli applied to the different cavities of a frog's heart, and the modifications of their effect by heat, cold, and the action of strychnia. The effect of electrical stimuli upon the ventricle, and the alterations occasioned in it by the application of heat, have already been studied by Professor Marey. The time relations of excitation in the frog's heart have also been very exactly determined by Dr. Burdon Sanderson and Mr. Page. But it seemed desirable to extend the scope of the research, and instead of confining ourselves like previous observers to the effect of stimulation applied to the ventricle alone, to observe also the effect of stimulation of the ventricle, auricle, and venous sinus, both on the ventricular and the auricular contractions. This we did with the hope that from such series of observations we might be able to arrive at some conclusions regarding the transmission of stimuli from one part of the heart to the other in the ordinary course of the circulation. Professor Marey found that when an electrical stimulus was applied to the ventricle of a pulsating frog's heart the effect differed according to the condition of contraction or relaxation in which the ventricle was at the time the stimulus was applied. During the first part of the contraction of the ventricle, from the commencement of the contraction until nearly its maximum, stimulation had no apparent effect at all, and this period Marey terms the "refractory period." Following this phase is a second one, to which we have given in the following paper the term of the "sensitive phase," lasting from the maximum of systole to its end. The refractory period varies in duration according to the intensity of the stimulus, and the conditions under which the heart is operated upon. The feebler the stimulus, the longer is the refractory period. When the stimulus is very slight the refractory period may persist during the whole ventricular systole; as the stimulus is increased, the refractory period becomes shorter, and finally, when it is very strong, disappears altogether.

Heat applied to the heart shortens the refractory period or abolishes it altogether. Cold has an opposite effect, and lengthens the refractory period. The contractions caused by artificial stimulation do not much alter the cardiac rhythm, for the accelerated beat is followed

by a longer pause than usual, which compensates for the diminished interval between the two first beats. Sometimes no ventricular contraction is induced, and then instead of acceleration there is apparent inhibition, the application of the stimulus being followed simply by a longer diastolic pause than usual.

Marey's observations were confined entirely to the movements of the ventricle, but we have extended ours to the movement of the auricle as well. We employed two levers: one resting upon the ventricle, and the other upon the auricle, which recorded movements upon a revolving cylinder covered with smoked paper.

It is unnecessary to enter here into a fuller description of the apparatus, which is given elsewhere.*

By the method employed we are able to study the effects of maximal and minimal stimulation applied to the ventricle, auricle, and venous sinus upon the movements both of auricle and ventricle.

By minimal stimulation we understand the smallest shock that produces any visible effect that in any way modifies the course of contraction or the rhythm of the organ; and by maximal stimulation we mean the electrical irritation of such a strength that its intensification produces no visible increase in its effect.

The apparatus for stimulation consisted of a bichromate battery with two zinc ($3\frac{1}{2}$ inches by 2 inches) and three carbon plates, the size of these being 8 inches by 2 inches. This was connected with a coil, and a key was interposed by which the primary circuit could be made and broken at pleasure. The moments of opening and closing the circuit were registered upon the same revolving cylinder as that upon which the cardiac pulsations were noted, by means of an electro-magnet, the marker of which was placed immediately under the pens of the cardiac levers. In all the tracings the upper curved line shows the ventricular contractions, the lower curved line the auricular contractions, and the broken straight line the moment of excitation. The descent of the line indicates the opening, the ascent the closing of the current.

In the secondary circuit were placed the electrodes for stimulating the various parts of the frog's heart, and this circuit also could be broken or changed at pleasure by means of an interposed double key.

The heart was stimulated by a single induction shock. In minimal stimulation only the breaking shock was effective, in maximal stimulation both making and breaking shocks. The apparatus, which is described in a separate note, admitted of the venous sinus, auricle, or ventricle being stimulated at will.

When recording the effects of stimulation of the venous sinus we speak only of changes in rhythm of auricle and ventricle.

We shall examine *seriatim* the results of irritation of each of these.

* Cash. "Journal of Physiology," vol. iv, No. 2.

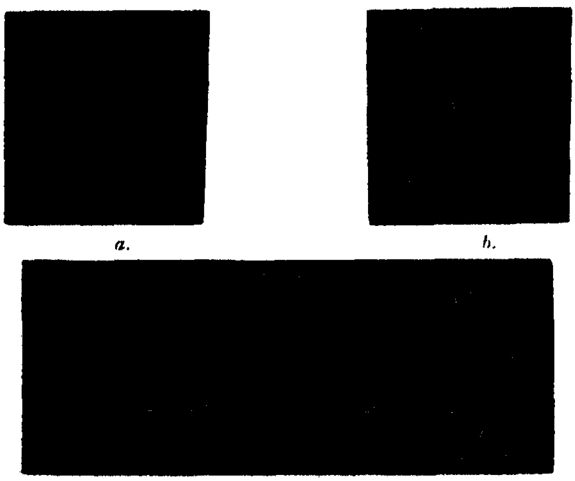
The temperature of the room in which the experiments were conducted was 67° to 70° F. The frog employed was, on all occasions, the *Rana temporaria.*

Stimulation of the Ventricle—Minimal.

On stimulating the ventricle with a single induction shock of minimal potency we find—

(1.) That between the commencement of the ventricular systole up to or nearly up to its maximum there is a refractory period (fig. 1,

FIG. 1.



a. *b.* *c.*

Stimulation of Ventricle (minimal).

a and *b*, stimulation in different phases of refractory period.
c, stimulation after refractory period has passed, showing different forms of reduplication.

a and *b*) during which stimulation applied to the ventricle has no effect whatever on that beat of the heart, or the one succeeding it, nor is the auricle in anywise affected.

(2.) That after the refractory period has elapsed stimulation causes a reduplication of the beat (fig. 1, *c*).

(3.) The latency of this reduplicated beat becomes distinctly shorter as the systole passes into the diastole. Thus supposing the value of a single cardiac systole to be 1″·3, stimulation falling just at the maximum of a beat will cause a reduplicated beat with a latency of ·33. When the stimulation occurs half way down the curve of relaxation, the latency is ·18 or ·2, and when applied at the instant before the abscissæ would have been reached the latency is only ·13.

(4.) Where acceleration or reduplication occurs, the subsequent diastolic pause is prolonged, so that the time occupied by the two beats, the interval between them longer or shorter, and the subsequent pause, is nearly equal to the time which would be occupied by two normal beats with their associated diastolic pauses (fig. 1, c).

(5.) The ventricular reduplication is often associated with a reduplication of the auricular beat, but in no case has the latter its commencement before the former. It is usually, in fact, distinctly later (fig. 2, *a*).

It is to be noted that, *minimal stimulation* applied to the *ventricle* during its refractory period produces *no* effect on the *auricle*.

FIG. 2.

a.

Stimulation of Ventricle (minimal). Tracing shows long pause after reduplication. The two opening stimulations occur after maximum of systole has passed.

Time-Writer, marking seconds. Applicable to all tracings in the paper, except those in the Appendices.

We may divide each ventricular cycle into three parts, the first reaching from the commencement of systole nearly up to its maximum, the second from nearly the maximum of the systole to its end, and the third embracing the whole diastolic period from the end of one systole to the beginning of another (*vide* diagram A) except

A.

Diagram A shows the division of the ventricular cycle into three parts.—
1. Refractory period. 2. Sensitive period. 3. Accelerative period.

when the stimulation falls immediately after the end of the refractory period. In all these points our results agree with those already obtained by Marey.*

* *Op. citat.*, p. 72.

Stimulation applied to the ventricle during the first period has no effect whatever either in accelerating the occurrence of the second beat, or altering the length of the subsequent pauses. This constitutes the refractory period.

Stimulation applied during the second period causes reduplication of the systole, the next systole succeeding with a constantly diminishing latency up to the end of the period. When the stimulation is applied in this period, the two systoles being more or less united, there is no distinct pause between them, but the diastolic pause succeeding the second occupies very nearly the interval of time corresponding to two normal diastolic pauses. In this second period the heart is more sensitive to the action of minimal stimuli than in the first period. In the third period, that of acceleration, stimulus applied to the ventricle hastens the advent of the succeeding systole, and the latent period is very short, being nearly equal throughout its whole extent to the latency at the end of the second period. The sensibility of the heart to stimuli is scarcely so great in this period as in the second.

The length of the diastolic pause succeeding the accelerated systole is longer than normal, the increase in length being nearly equal to the amount of acceleration.

Stimulation of the Ventricle—Maximal.

When stimuli of maximal potency are applied to the ventricle between the maximum auricular systole and the commencement of ventricular systole, the ventricular systole immediately following the stimulus is rarely slightly higher than normal, and the diastolic pause succeeding it is excessively long—so long, indeed, as to be nearly, if not quite, equal to the time which would, as a rule, be occupied by two diastoles, so that the time occupied by the systole and diastole after stimulation applied at this period of the heart's cycle, is equal to the time usually occupied by one systole and two diastolic pauses.

In most cases this systole was apparently no higher than normal, and consequently we cannot with plausibility regard it as a case of superposition of two systoles.

In some cases the time within which this pause may be produced is strictly limited to the point indicated; in others, however, it may extend some little distance towards the maximum of systole, though it never reaches this. In other words, it may encroach upon the refractory period which we have mentioned when speaking of minimal stimuli, although it never extends through the whole of it.

This phase may occasionally, though rarely, be absent. Its place is then taken by reduplication, or very rarely by insensibility to stimulation, as in the refractory period.

Reduplication with maximal stimuli occurs during all times of the cycle, except at the very commencement of the systole.

A very considerable latency is to be observed in cases where stimulation falls early in the systole. The latency, when this is the position of the shock, is usually ·5′ or even more, and occasionally where stimulation is coincident with the earliest possible attempt at systole, nearly the whole beat may lapse before reduplication.

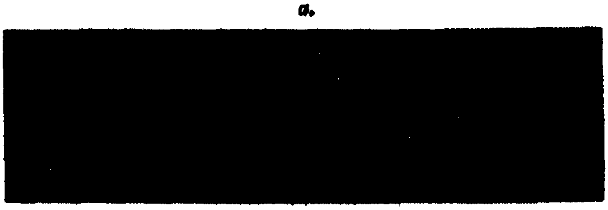
The latency is greatest when the stimulus is applied at the commencement of the ventricular systole (with the exception of its very beginning), and it gradually decreases towards the end of systole, at which time it is at a minimum. During the diastole the latency seems to remain constantly the same as at the end of systole. The later in the phase of ventricular activity the reduplicated systole commences the more perfect is it.

In all the points already mentioned our results agree with those of Marey.

Stimulation of the ventricle falling before or at the maximum of ventricular systole, *i.e.*, during the refractory period of a minimal stimulation, frequently causes a reduplication of the auricular systole which holds the same relation to the induced ventricular beat that the auricular contraction normally holds to the ventricular.

FIG. 3.

a.



b.

c.

Stimulation of Ventricle (maximal).

a, normal tracing; *b*, effect of maximal stimulation. In *b* inhibition is seen.

Stimulation falling after the maximum of ventricular systole may cause an induced auricular contraction, but this is nearly synchronous with, or even subsequent to, the induced ventricular contraction (fig. 4, *a* and *b*, and fig. 3, *c*).

Fig. 4.

a.

b.

Stimulation of Ventricle (maximal).

Sometimes reduplication of the ventricular beat may occur without reduplication of the ventricular (fig. 3, *c*).

These results may be possibly due, in part at least, to direct stimulation of the auricle itself by the strong current used to stimulate the ventricle.

Stimulation of the Auricle—Minimal.

When minimal stimuli are applied to the auricle, there is occasionally a refractory period, extending from the beginning to the maximum of auricular systole. When the stimulus is applied at the maximum of auricular systole, or just after it, it sometimes produces an omission, or, as we may term it, an apparent inhibition of the next auricular and ventricular systoles (fig. 5, *b*). Stimulation falling after this point and occasionally on it, will cause a reduplication of auricular and ventricular contractions to occur which may have a latency of as much as 1·25 seconds.

Fig. 5.

a.

b.

Stimulation of Auricle (minimal).

No secondary contraction can usually be produced in the ventricle till an induced auricular contraction has occurred; and as the auricular latency is considerable, the ventricular latency is also very long. Thus, should the stimulus producing contraction fall at the commencement of ventricular systole, the auricle may have a latency of one second and the ventricle of 1·4 seconds.

The sensibility of the auricle to minimal stimulation may generally be divided into three phases:—

1stly. Stimulation may fall at such a stage of auricular activity that it does not cause an instantaneous response, but allows the auricle to pass through its diastole before it causes reduplication.

2ndly. It falls at such a time that the auricle responds instantaneously.

3rdly. About or shortly after the period of auricular maximum, stimulation may cause inhibition of the auricular and the ventricular sequential beat.

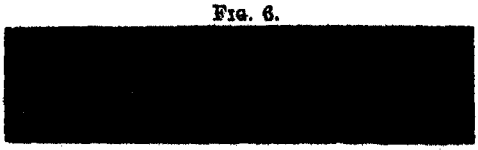
Stimulation at any period during the diastole of the auricle until the abscissa is reached, causes a reduplication. The latency of this reduplicated beat is shorter the further the diastole is advanced. It is followed by an induced ventricular beat in ordinary rhythm.

Stimulation during complete auricular diastole and before systole commences causes a contraction with very short latency, succeeded by an induced ventricular contraction. But it is to be noted that occasionally stimulation at this period causes a normal auricular contraction with an appreciable latency, and followed by a ventricular contraction.

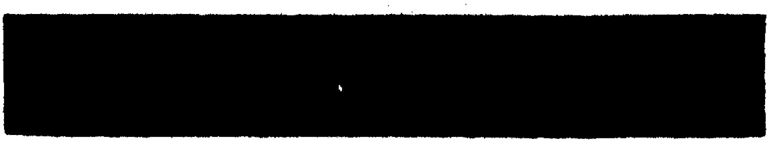
Stimulation of the Auricle—Maximal.

Maximal stimulation of the auricle almost always produces some effect both on the ventricular and auricular beat: this effect is usually one of stimulation, but it may be of apparent inhibition.

FIG. 6.



a.



b.

Stimulation of Auricle (maximal).

Maximal stimulation usually induces a ventricular beat whenever it is applied (fig. 6, *a*), excepting when it falls just after the summit of the auricular contraction.

Stimulation at this point may cause no auricular contraction, but on the contrary may induce omission of the subsequent auricular and ventricular beat (fig. 6, *b* 2).

When an auricular beat has been induced by stimulation, it is followed in the ordinary way by a beat of the ventricle, excepting when the stimulus is applied to the auricle just at the commencement of the ventricular systole. In this case an auricular beat may be induced, which instead of being followed by a corresponding ventricular one, *is followed, on the contrary, by an omission of the ventricular beat* (fig. 6, *a* 1).

At this point the latent period may be looked upon as indefinitely long, as stimulation produces no contraction at all.

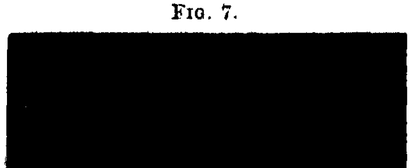
The more closely after this point stimulation is applied the longer is the ventricular latency.

Stimulation of Venous Sinus—Minimal.

The venous sinus appears to be more sensitive to stimulation than either auricle or ventricle, so that stimuli applied to it produce an effect, although they are much slighter than the minimal stimuli of either auricle or ventricle.

Stimulation of the venous sinus by a minimal shock is usually potent to produce some effect or other at every stage of ventricular activity (fig. 7, *b*).

FIG. 7.



a.

b.

Stimulation of Venous Sinus (minimal). In neither *a* nor *b* is the closing shock effective.

Stimulation at the instant of commencement of ventricular systole

usually causes omission of the following sequential beat of both auricle and ventricle.

This period may occasionally be slightly prolonged into systole.

Stimulation of the sinus at all other periods of ventricular activity causes a reduplication of the systole. This induced ventricular systole is preceded by an induced auricular systole, and therefore has the prolonged latency before referred to.

Stimulation falling at the commencement of ventricular systole may cause auricular reduplication with ventricular omission (fig. 7, *a*).

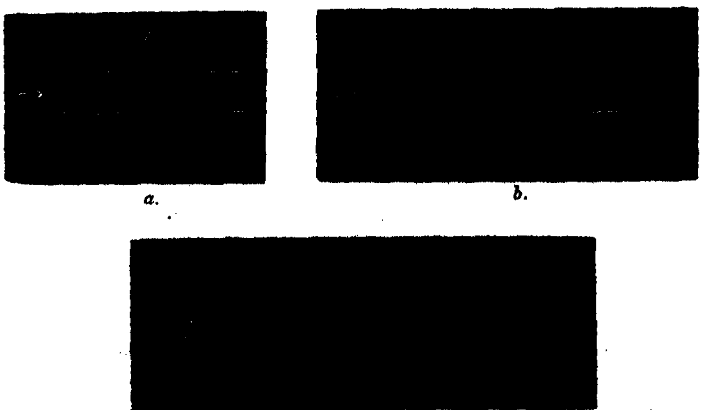
In consequence of the long latency, we find all ventricular curves separated by a distinct interval from (their) reduplications.

Stimulation of Venous Sinus—Maximal.

The period during which stimulation causes ventricular omission is well marked, and in some cases extends into the ventricular systole as it advances towards its maximum, though the effect is never produced at the maximum.

This omission of ventricular beat is most usually associated with a reduplication of the auricular beat, the second auricular contraction occurring within that ventricular systole at the commencement of which the shock was communicated (fig. 8, *b*).

FIG. 8.



a. *b.* *c.*

Stimulation of Venous Sinus (maximal). *a*, normal rhythm; *b* and *c*, stimulations all effective.

Reduplication occurs in all phases except at the period when stimulation causes omission. The latent period of this reduplication is usually short, as in the case of a ventricle stimulated directly, inasmuch as the induced auricular contraction does not precede the

induced ventricular, except when stimulation falls before the maximum of ventricular systole, in which case there is usually a regular sequence of auricular and ventricular contraction (fig. 8, *c*).

Usually after the maximum of ventricular systole stimulation causes a reduplicated beat with short latency, inside of which curve falls that of the induced auricular contraction; however, genuine sequential reduplication of auricle and ventricle with long latency is not uncommon. Not unfrequently, after repeated stimulation of the sinus, the heart assumes a new rhythm, which may be twice as rapid as it was originally, and though omission of the alternate beat may still be produced by stimulation at the time already indicated, the organ returns again to its accelerated pace. In time, if stimulation be withheld, the rhythm lapses again into the normal. The auricle shares in the ventricular excitement (fig. 9).

FIG. 9.

Rhythm which has been changed by repeated stimulation of Sinus returning to normal.

THE EFFECT OF COLD ON THE FROG'S HEART.

In these experiments the animal was placed upon a wire gauze grating, and covered with a small bell-jar. Underneath the grating and around the bell-jar was placed ice, so as to surround the frog, which was kept in this position for an hour or longer. When its movements had become slow and torpid it was killed, without loss of blood, and placed on the cardiograph, already described, the temperature in the vicinity being kept low by means of blocks of ice placed on the metal bars supporting the animal. The apparatus was employed in the same manner as in our observations on the effect of electrical stimuli on the normal heart, and the same order was observed in recording the results.

Effects of Electrical Stimulation of the Ventricle—Minimal Stimuli.

The contraction of the chilled frog's heart, as is well known, lasts for longer time than in the ordinary condition. When minimal stimuli are applied to the ventricle (fig. 10) it is found that there is a distinct refractory period, extending from the beginning of systole up

to the last third of the summit of the curve in the accompanying tracing, and persisting past the maximum of systole.

FIG. 10.

Stimulation of the Ventricle (minimal). Opening stimulation only effective.

It is, therefore, always longer than in the normal heart. After the refractory period has passed, stimulation causes reduplication of the ventricular beat. The later on in the diastole that the stimulus falls the shorter is the latency.

Ventricular Stimulation—Maximal.

Stimulation sometimes causes omission when applied at the very commencement of systole. All stimulation thereafter applied causes a reduplication of the ventricular systole, with a latency that becomes shorter the later the stimulation is applied. If auricular reduplication occurs it is always sequential to ventricular (fig. 11, *a*). Stimulation

FIG. 11.

a.

b.

Stimulation of the Ventricle (maximal).

at the maximum of systole may cause a blending with the reduplicated beat closely resembling one prolonged systole (fig. 11, *b*). The induced beat is most perfect when stimulation falls, just as the abscissa is

reached. Stimulation before the maximum of systole has longer latency than stimulation at the maximum.

Auricular Stimulation—Minimal.

A refractory period is obviously present, but it is not of so great length as in the case of ventricular stimulation. It may be said to extend usually through the maximum of auricular systole (fig. 12), and even up to near the maximum of ventricular systole; occasionally it exists only just at its commencement.

FIG. 12.

Stimulation of Auricle (minimal).

As regards the reduplication, we find that as in the case of the normal heart, a long latency prevails, because an induced auricular systole must occur before the ventricle contracts again. But if the stimulation fall just at the time when the abscissa is reached, or rather before this point, a ventricular contraction may exceptionally be produced with a short latency, and the auricular induced contraction succeeds it.

Auricular Stimulation—Maximal.

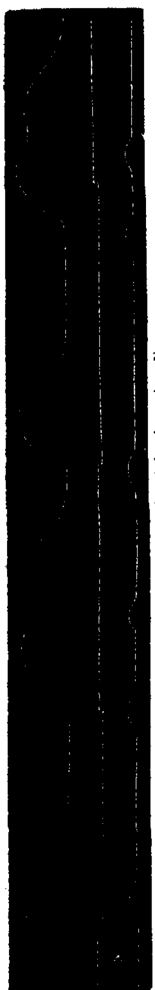
The same features are to be observed as in the last section, except that there is no refractory period (fig. 13).

Stimulation in all phases of the ventricular cycle usually causes a reduplicated auricular and ventricular beat. Should the stimulation fall before the ventricular maximum is attained, the auricular reduplication precedes the ventricular in the ordinary way, but should the stimulation fall, after the ventricular maximum, the auricular reduplication is exceptionally synchronous with, or subsequent to, the ventricular: usually, however, the induced ventricular beat precedes in ordinary rhythm the induced ventricular.

Stimulation of the Venous Sinus—Minimal.

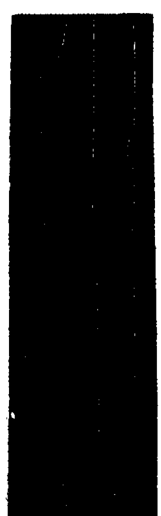
A refractory period may be present on minimal stimulation, nearly up to the maximum of ventricular systole. Thereafter reduplication results. A strictly minimal stimulation may originate a reduplication at any period of the beat having a long latency, that is to say,

Fig. 13.



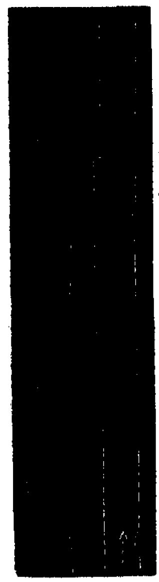
Stimulation of Auricle (maximal).

Fig. 14.



Stimulation of Venous Sinus (minimal).

Fig. 15.

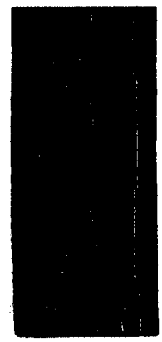


Stimulation of Venous Sinus (maximal).

Fig. 16.

Stimulation of Ventricle (minimal). Auricular tracing omitted.

Fig. 17.



Stimulation of Ventricle (maximal).

the ventricular reduplication is preceded by the auricular induced contraction (fig. 14). Thus stimulation applied just before the ventricular relaxation is completed, instead of having an instantaneous ventricular and auricular response resulting from stimulation of auricle or ventricle, has a long latency, wherein the auricle reduplicates. The further on in the systole the stimulation is applied, the shorter is the latent period, and the more perfect the reduplicated contraction.

Stimulation of the Venous Sinus—Maximal.

Omission may be caused by stimulation applied at the commencement of systole, or reduplication may occur in all phases (fig. 15).

Reduplication has the longest latency at the commencement of systole, and there is true auricular precedence up to or beyond the maximum in this phase. In the decline of systole, after the maximum is passed, and the abscissa has been nearly reached, there is occasionally reduplication with short latency, the auricular and ventricular contractions being synchronous.

ACTION OF HEAT ON THE HEART.

In this series of experiments the pithed frog in which the brain and spinal cord had been destroyed, was laid upon a metal plate, the temperature of which was gradually raised by means of a flame beneath it.

Ventricular Stimulation—Minimal.

The refractory phase is generally wanting in the ventricular systole, but it may be present in exceptional cases, not unfrequently in the same tracing in which stimulation most generally produces reduplication (fig. 16).

It may be noted that irregularity of response to stimulation is one of the characteristics of the heated condition. Stimulation usually causes reduplication. Should stimulation fall at the commencement of ventricular systole, no effect is produced till the whole cycle of the systole has been passed through, when reduplication by a very perfect systole occurs. Latencies diminish in proportion as the stimulation occurs later in the systole of the heart. Reduplication occurring in response to stimulation falling at the maximum is often demonstrated by a beat originating when the relaxation after systole is completed, and therefore distinct from the original beat: this is due to the fact that the curve of the heated heart is much shorter in duration, and therefore the reduplication falls outside the systole during which stimulation occurs, the latency being actually shorter, however, than in the unheated heart.

Ventricular Stimulation—Maximal stimuli.

When stimuli of maximal intensity are applied to the ventricle of the heated heart, we notice (fig. 17) :—

(1) That there is no refractory period; (2) Stimuli at the commencement of the ventricular systole may cause omission of the succeeding beat; (3) Reduplication occurs at all phases, and has the same characteristics as in minimal stimulation; (4) Latencies follow the same rule as in minimal stimulation; (5) The reduplicated beat is most perfect when stimulation falls—

I. At the very commencement of systole.
II. At its termination.

The value of any beat and its reduplication, with the time intervening and of the succeeding pause, was about equal to two normal cardiac cycles. Occasionally a double reduplication, or a series of contractions, resulted from a single stimulation.

Auricular Stimulation—Minimal Stimuli.

There is apparently no refractory period. All stimuli cause reduplication, and in all cases induced auricular systole precedes an induced ventricular systole. This occurs even in advanced auricular diastole, when occasionally in the normal heart a simultaneous auricular and ventricular systole results.

Auricular Stimulation—Maximal Stimuli.

There is no refractory period. Stimulation just after the auricular maximum has been passed frequently causes an apparent omission of the following beat.

Stimulation before the maximum of the ventricular systole causes an induced ventricular beat preceded by an auricular contraction.

After the maximum, stimulation usually has the same effect, but occasionally causes an instantaneous reduplication of both auricular and ventricular beats.

A reduplicated ventricular beat is of the character already described.

Stimulation of the Venous Sinus—Minimal Stimuli.

The venous sinus in its general absence of a refractory phase shows a resemblance to the ventricle, but it may manifest the same exception in exhibiting it.

When this occasional refractory period is present it may exist during active systole, and up to its maximum. It is exceptionally present in cases which as a rule show no refractory period.

Stimulation falling before the maximum of systole (fig. 18, a ventricular tracing alone given) causes a reduplication which is preceded by

Fig. 18.

Stimulation of Venous Sinus (minimal).

an auricular contraction, whilst stimulation falling immediately after maximum of systole causes reduplication, which may be preceded by an auricular pulsation, or may occasion an induced systole, auricle and ventricle contracting at the same time.

The most perfect reduplicated beat occurs when stimulation falls at the end of systole.

Venous Sinus—Maximal Stimulation.

Occasionally a stimulation of maximum strength falling at the commencement of the ventricular systole causes an apparent omission of the following pulsation; but this result is not so frequent as in the case of the normal heart. Usually a distinct reduplication occurs at whatever time in the cycle stimulation falls (fig. 19).

Fig. 19.

Stimulation of Venous Sinus (maximal).

The reduplication is at all times, except in the last stage of systole, preceded by an auricular contraction.

The auricular induced contraction appears to follow stimulation more rapidly than in the case of the normal heart. Therefore the induced ventricular contraction (fig. 19, ventricular tracing alone given) which follows the auricular has a shorter latency than is normally the case. The heating process having been carried so far that a rapid cardiac rhythm with imperfect systole has resulted, it is often found that there is an indifference to stimulation in the so-called refractory period, or even in all phases of the cardiac cycle alternating with the usual sensibility.

On the Effect of Strychnia upon the Frog's Heart.

The apparatus used in this series of experiments was identical with that employed in the investigation of stimuli applied to the frog's heart. The frog was killed by the brain being destroyed, and a small

dose of strychnia was then introduced into the dorsal lymph sac. As soon as the effect of the drug upon the spinal cord was evidenced by distinct spasm, the heart was rapidly exposed, placed on the cardiograph, and stimulation applied. The same order will be observed as in the description of the experiments on the normal heart.

Stimulation of the Ventricle—Minimal.

On applying a minimal stimulus to the strychnia heart (fig. 20) we were struck, in the first instance, by the extreme length of the

Fig. 20.

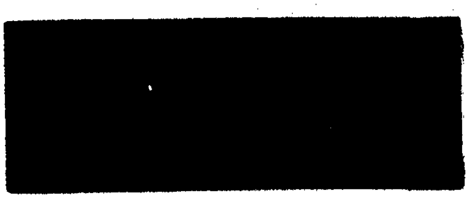
Stimulation of Ventricle (submaximal).

refractory period. Stimulation has usually no effect, not only when applied before the maximum of the systole as in the normal heart, but also in the maximum, and often far into relaxation. After the phase has passed the stage of reduplication ensues. Reduplication is very complete; its latency becomes diminished as diastole advances. The reduplicated ventricular beat is succeeded by an auricular pulsation. After the customary pause, the heart resumes its wonted rhythm. It is rarely that stimulation falling at the commencement of ventricular systole causes inhibition. If the auricle is unstimulated and its rhythm is unaltered, there is short latency for the ventricular reduplication. If the auricle is stimulated and contracts before the ventricle, there is long latency, but the latter is rarely seen when a refractory phase is present.

Ventricular Stimulation—Maximal Stimuli.

In this case no refractory period exists. An inhibitory period exists occasionally but with this exception, all stimulation produces redupli-

Fig. 21.



Stimulation of Ventricle (maximal).

cation (fig. 21). Should stimulation fall at the commencement of systole, the latency is long, nearly equal to the length of the beat; and the reduplication is very complete.

Stimulation at the maximum of systole has a latency of about two-fifths of a second, and thereafter during the subsidence of the ventricle, the period of latency rapidly diminishes. The most perfect beat of reduplication is produced by stimulation at the commencement of systole, or after relaxation of the ventricle.

Stimulation of the Auricle—Minimal.

It is but rarely that we see a refractory period whilst applying minimal stimuli to the auricle. Usually, stimulation at all times causes a reduplicated beat, the auricular reduplication preceding that

FIG. 22.

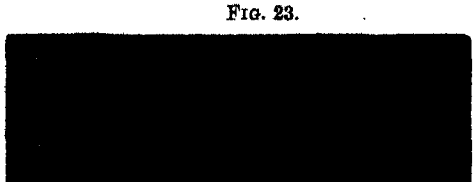
Stimulation of Auricle (minimal).

of the ventricle in the usual rhythm. On stimulation, an auricular systole is produced, and not until this movement has reached the usual point does the ventricle commence its systole (fig. 22).

Stimulation of the Auricle—Maximal.

There is no refractory period. Occasionally the stimulus falling at the very commencement of the ventricular systole will cause inhibition or coincidence of the following beat, or it may cause a reduplication with a latency of about one second (fig. 23).

FIG. 23.



Stimulation of Auricle (maximal).

The latencies are invariably long when the auricle is so stimulated that its induced beat is a normal one and precedes the induced ventricular systole in its normal rhythm.

Stimulation of the Venous Sinus—Minimal Stimuli.

With minimal stimulation of the venous sinus there is no refractory period except occasionally for an instant at the maximum of auricular systole. Reduplication occurs at all phases of the ventricular systole. Length of latency depends upon whether the stimulation induces an auricular contraction or not. If the auricular systole follows the stimulus, then the ventricular latency must be long (fig. 24).

Fig. 24.

Stimulation of Venous Sinus (minimal).

* It is longer if stimulation falls at the commencement of ventricular systole, because at that phase, until the auricle has contracted, no ventricular reduplication occurs. Occasionally, though rarely, and that after the maximum of ventricular contraction, auricular reduplication follows, or is synchronous with the ventricular systole, and then latency is invariably short. Reduplication with prolonged latency, probably from auricular reduplication, is well seen in the appended tracing (fig. 25).

Fig. 25.

Stimulation of Venous Sinus (minimal).

The stimulation at the end of relaxation in one case causes reduplication of auricle, coinciding with that of the ventricle.

The tracing illustrates the rule that when the sinus is stimulated no refractory period is observed as regards the ventricular reduplication.

Venous Sinus—Maximal Stimulation.

There is no insensitive period as far as regards the ventricle. During all phases of the systole stimulation causes a reduplication of the

ventricular beat (fig. 26). Inhibition of the ventricular systole has not been found in many of the hearts examined, though it occasionally occurs.

Fig. 26.

Stimulation of Venous Sinus (maximal).

Should the exciting shock fall at such a time as to cause an instantaneous auricular systole, we find this systole is nearly synchronous with that of the ventricle, and that the latter has a short latency, but should the shock fall so that the auricle responds by a genuine contraction, the ventricular reduplication follows with a long latency.

Inhibition of the ventricular with reduplication of the auricular beat may result occasionally from stimulation of the venous sinus.

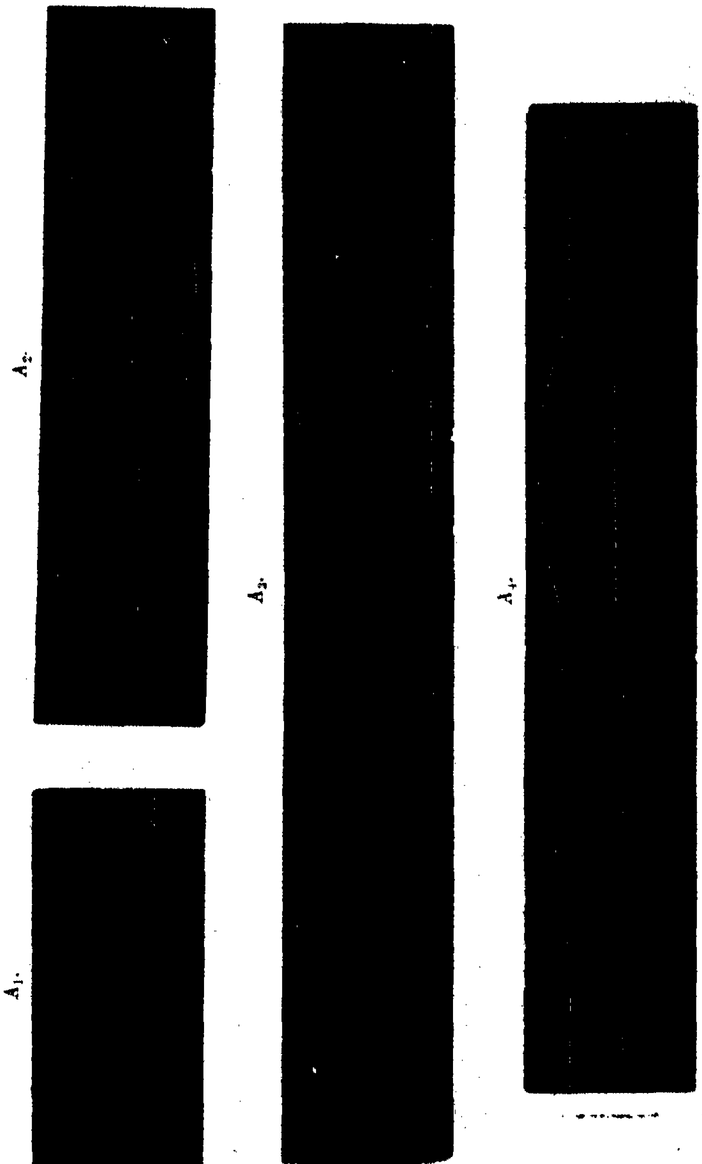
Appendix A. Cooled Heart.

The construction of a simple piece of apparatus has enabled us to obtain curves much more striking than those which appeared in the foregoing paper, as they represent a far greater variation of temperature.

Instead of the gutta-percha support for the heart already described, a hollow copper pan of similar shape was employed. It was provided with influx and efflux tubes, and insulated below by a plate of ivory in which ran also the electrodes destined for the stimulation of the sinus. This was connected with the usual support passing over the body. Upon minimal stimulation of the ventricle itself the succession of auricular and ventricular contraction is illustrated in the charts A 1–4 here inserted. It is seen that the action of cold modifies considerably the relation between the ventricular contraction and the succeeding auricular beat. In A_2 we find a reduplicated ventricular beat succeeded by a normal auricular contraction.

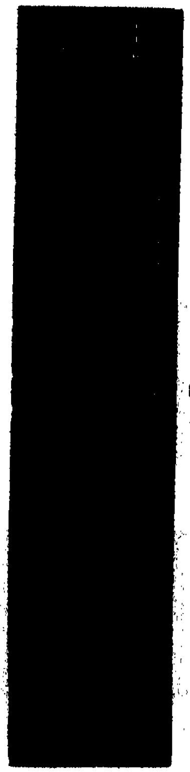
In A_3 cooled through about 2·5° C., the ventricle responds to the same stimulation, and the wave does not pass upward to the auricle; and in A_4, in which the contraction and relaxation of the heart had become very slow from a further reduction of 2°, we find the auricular rhythm is regular in spite of ventricular reduplication. There is in

A_1 and A_2 an indication of aortic expansion; it is to be noticed that after the reduplication in A_3 this is omitted.

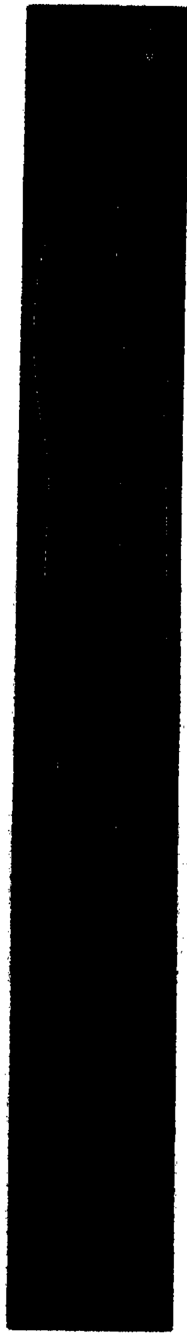


A_1. Normal Heart tracing.
A_2. Stimulation of Ventricle of the normal Heart.
A_3. Stimulation of Ventricle of Heart cooled through 2°·5 C.
A_4. Stimulation of Ventricle of Heart cooled through 4° C.

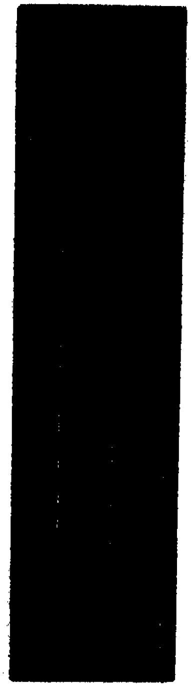
B_1.



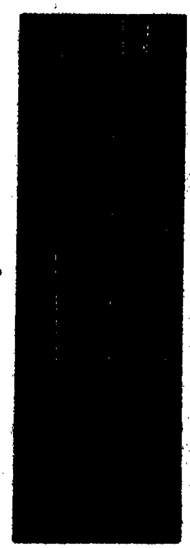
B_2.



B_4.



B_3.



B_1. Cold. Maximal Stimulation of Auricle.
B_2. Considerable cold. Maximal Stimulation of Auricle.

B_3. Cold. Maximal Stimulation of Auricle.
B_4. Cold. Maximal Stimulation of Auricle.

Auricular Stimulation.

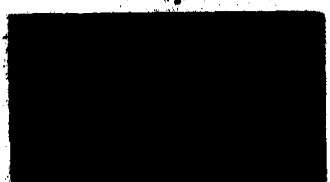
Many additional experiments upon cooled hearts have tended to show that it is very rarely that stimulation of considerable strength calls forth a ventricular beat, preceding or coexistent with the auricular. Usually at all phases of stimulation which cause a reduplication of the auricular beat, the ventricular succeeds in normal relationship (B 1 and 2). There is an exception to this, however, which is frequently demonstrated; this is, that whilst the auricular beat is reduplicated the ventricular is not, but is succeeded by a long diastolic pause (B_3), after which the auricle takes up its old rhythm. Still more rarely stimulation just before commencement of ventricular systole causes omission of both succeeding auricular and ventricular beats (B_4).

The latency of reduplication varies considerably in minimal stimulation of the auricle, but this variation is not so much owing to loss of time in the auricular as in the ventricular reduplication. Thus in C_1 stimulation at the end of auricular relaxation, ventricular latency

C_1.

C_2.

C_3.



C_1 to C_3. **Levers as in A. Auricular Stimulation (minimal) of Cooled Heart.**

is 1″·2, in C_2 stimulation halfway to ventricular maximum latency is 1″, and in C_3, when stimulation is at ventricular maximum, the latency is for the ventricle only 6″.

On the other hand, more powerful stimulation of the auricle causes reduplications of the ventricle, which are at all times of equal or of very slightly differing values. Thus in a heart much cooled (D 1 and 2) we have towards the commencement of ventricular systole and towards the end of relaxation a latency for the induced ventricular beat of 1″·2.

D_1.

D_2.

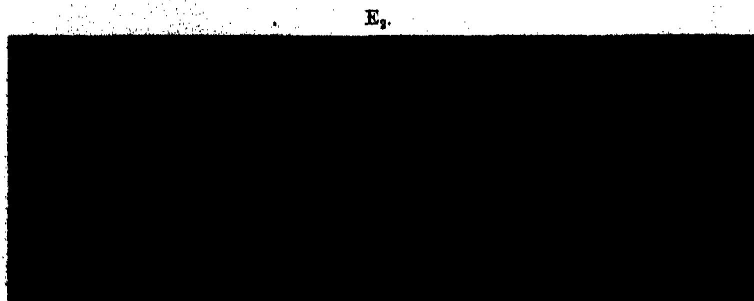
D_1 to D_2. Levers as in A. Auricular Stimulation (maximal) of Cooled Heart.

Venous Sinus.

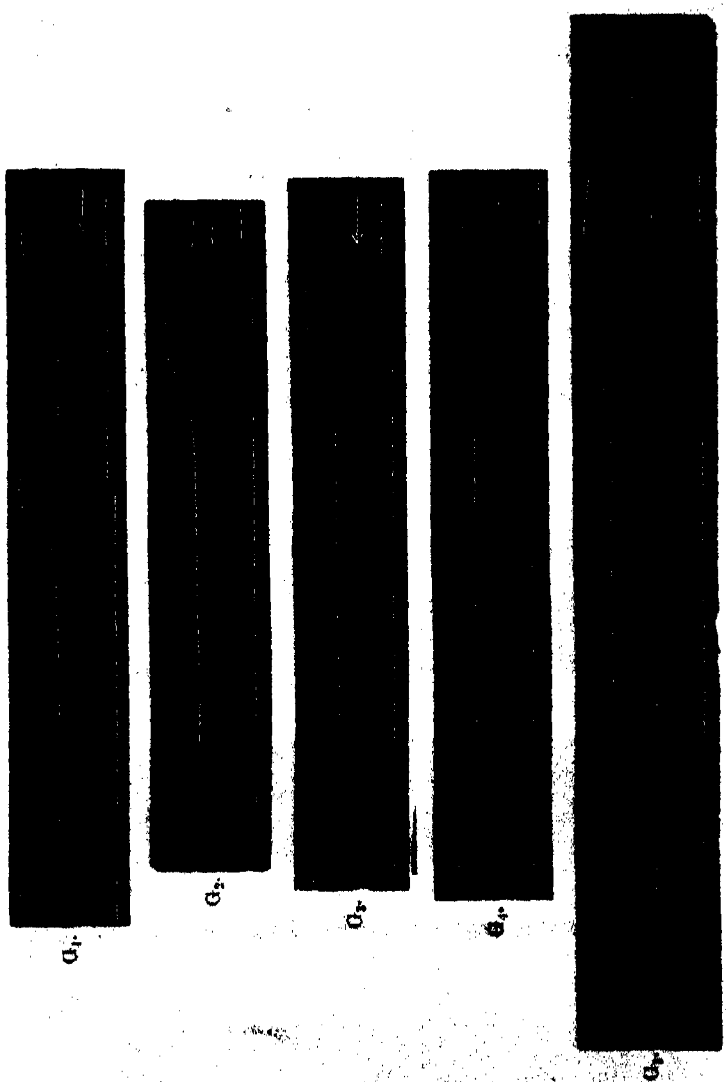
In the heart which has been moderately or slightly cooled, 4—6° C., the occurrence of ventricular reduplication without a precedent auricular reduplication is very rare, even when strong stimulation is employed. The refractory period occasionally observed may disappear after a few stimulations have been given, or it may persist. Further-

E_1.

E_2.



E_1 to E_2. Levers as in A, but no auricular tracing given. Stimulation of Venous Sinus. E_1, before cooling; E_2, after cooling.



Levers as in E. Stimulation of Venous Sinus. G_1 to G_5, gradual cooling of Heart.

more on cooling a heart which has at a certain temperature, E_1, shown a refractory period, we may find this converted into a period during which stimulation causes an omission of the following beat, E_2.

The duration of the diastolic pause is markedly influenced by temperature, whereas it appears to be but slightly affected by variation in the instant of stimulation by which it is produced.

In G_1 water of melted ice had passed through the support for two minutes.

G_2 water had passed 5′.
G_3 " " 10′.
G_4 " " 20′.

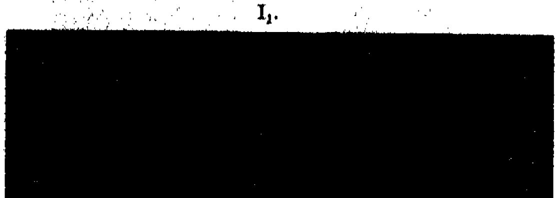
G_1 Duration of contraction	1″·4 ..	Length of pause from stimulation .. 4″·2
G_2 " " ..	1″·9 ..	" " 5″·6
G_3 " " ..	2″·2 ..	" " 6″·0
G_4 " " ..	2″·4 ..	" " 6″·2

G_5 was obtained from a heart cooled for a considerable time, and shows a remarkable prolongation of the systole and diastolic pause.

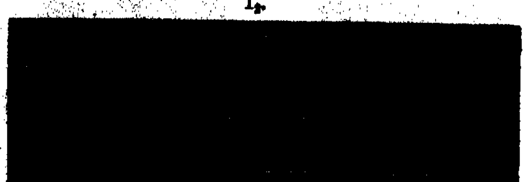
G_5 Duration of current 4″·4 .. Length of pause from stimulation.. 10″

It will be seen in all these cases that there is a certain relationship between the length of the contraction and of the pause.

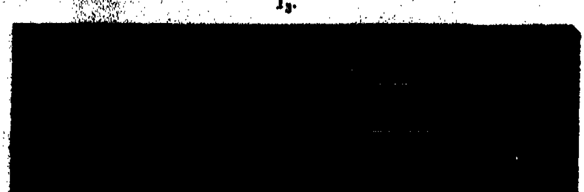
I_1.



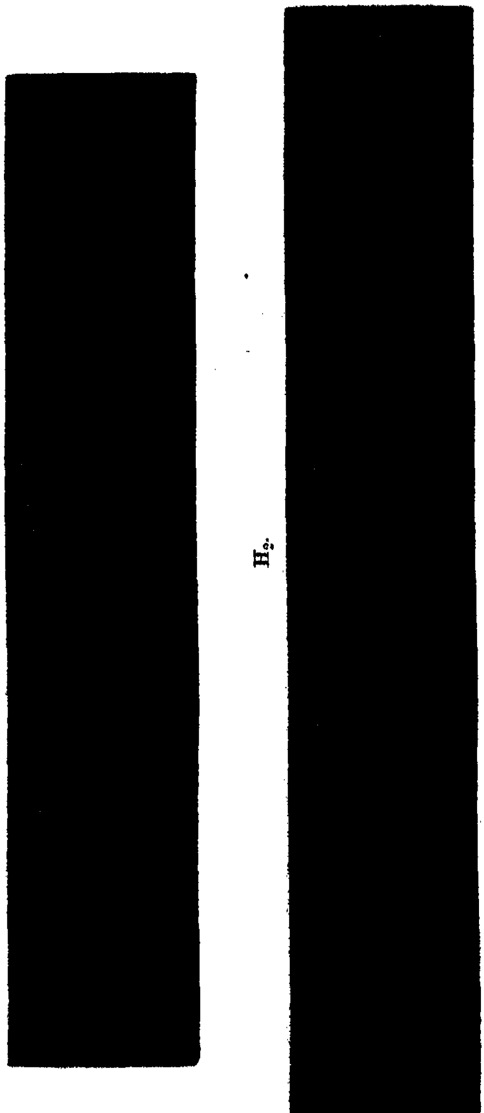
I_2.



I_3.



I_1 to I_3. **Levers as in A. Stimulation of Venous Sinus (maximal).**



H_1. H_2.

H_1 to H_2. Levers as in A. Stimulation of Venous Sinus (minimal).

The reduplication of the ventricular beat varies in regard to the time of stimulation under minimal stimulation. Thus in H when stimulation falls near the commencement of systole, auricular reduplication occurs in 1″·8 and ventricular reduplication in 3″·2. But in the same heart a stimulation during a period of relaxation yielded an

instantaneous auricular response, the ventricular reduplication occurring 1''·3 after stimulation.

In the case of maximal stimulation, the usual result is an instantaneous auricular systole succeeded by a ventricular. The latter, therefore, has a latency equal to the auricular beat; this is seen in I_1, I_2, in both of which the latency is about ·7''; but in I_3 we have, on the other hand, no auricular reduplication for 1''·1. In both this instance and H_1 stimulation occurred at the commencement of ventricular systole.

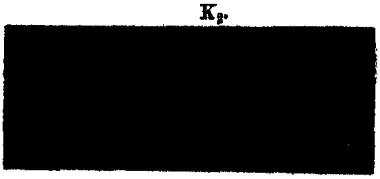
Time tracing. Electro-magnet recording seconds. Applicable to all tracings in Appendix A.

Appendix B. Heated Heart.

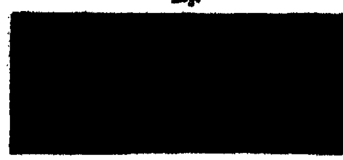
In some cases, heating a frog's heart through 4°·5 C. may fail to obliterate entirely the period of resistance to stimulation. Heat, however, in the same experiment may be shown to shorten the refractory period much, and to limit it to the very commencement of ventricular systole (in stimulation applied to the ventricle). The series of tracings given were taken from a large specimen of *Rana esculenta*, which had been kept at a low temperature for a considerable time before the experiment. The tracing obtained at room temperature (K) is therefore that of a cold heart, and the refractory period extends up to the commencement of relaxation after systole. After hot water had been run through the support for 5', and the temperature raised about 2° C., we find diastole increased and systole much shortened; at the same time there is a refractory period as extensive as in the cold heart, that is to say, extending to the commencement of relaxation K_2. In K_3 after heat had been applied 10', and the temperature raised another degree, stimulation at an earlier phase produces reduplication. Heated still further, K_4, there is reduplication at systolic maximum, and at K_5 everywhere except at the very commencement of systole. After heating through about 5° we still have a refractory period, whilst the curve has been reduced from 1''·4 to ·4''. In many cases, however, the same extent of heat may obliterate the refractory period completely. The heart which yielded these curves passed into rigor without showing the abolition. In the heated heart, of which the ventricle is stimulated, we may find that the auricle does not in any way participate in the ventricular excitement, but continues to beat in its usual rhythm. Thus, when the heated heart yields a series of contractions in answer to a single stimulation—a result not unfrequently obtained—the auricle does not reduplicate, but may

K_1.

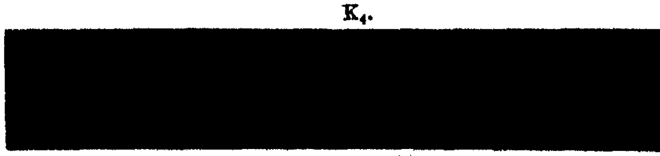
K_2.



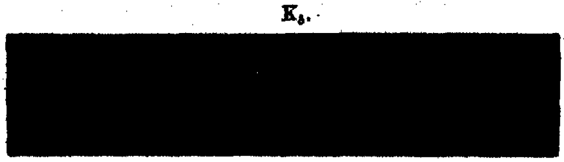
K_3.



K_4.



K_5.



Stimulation of Ventricle (maximal stimulation).

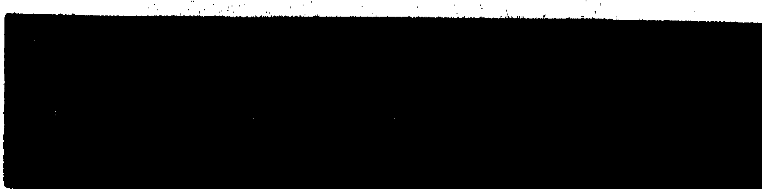
K_1. Heart at room temperature, frog long kept in cold room.
K_2. Temperature raised 2° C.
K_3. Taken 10′ later than K_2, during which time temperature was raised 1° C.
K_4. Temperature raised again 1° C.
K_5. Temperature raised 1° C., making about 5° C. altogether above K_1.

[1] Drum of more rapid rotation used in tracings given in Appendix B. The electro-magnet marks seconds.

give its systole in due place, whilst the ventricular contractions are still occurring. Not only is this indifference to ventricular action observed on the part of the auricle, but the counterpart may be occasionally seen in the ventricle, failing to follow the normal systole of

the auricle L. This is in part due to the fact that the auricle has only shared imperfectly in the heating.

L.



Stimulation of Ventricle (maximal). Heart heated about 7° C. Time as in K.

There is thus a disturbance of both muscle-wave and nervous impulse produced by the heating to which the auricles and ventricle have been exposed. This failure on the part of the ventricle occurs only after there has been a reduplication of its beat, and does not often occur, so far as we have seen, when stimulation, applied to the auricle itself (M), originates a systole there, for then the

M.

Stimulation of Auricle (maximal). Heart heated about 6° C.

ventricle follows in due course; we should therefore regard the exhaustion of the ventricle after its unusual activity as the cause of its quiescence after the normal auricular beat. Should stimulation be applied to the auricle during ventricular diastole, a reduplicated auricular beat succeeded by a ventricular at once occurs. In all phases this natural sequence is maintained, though sometimes at the end of its systole the auricular reduplication may be ·5″. Whilst a long pause follows this reduplication, it is very rarely that a stimulation of the auricle produces omission of the succeeding auricular and ventricular reduplication.

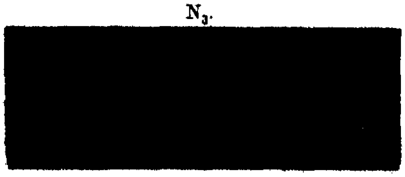
In stimulating the venous sinus, however, omission of the following ventricular beat is frequently produced when the shock falls at the commencement of ventricular systole (N_1), but we may find that there is an impulse propagated to the auricle, for this may reduplicate whilst the ventricle remains quiescent (N_2).

A little later, and up to the maximum of systole, the auricular

N_1.

N_2.

N_3.



Stimulation o Venous Sinus (maximal).

reduplication is succeeded by a ventricular (N_2), and after the maximum, and during the diastole of the ventricle, the induced auricular beat may occur synchronously with the ventricular, or it may precede it in regular course.

Both of the charts N_1 and N_2 are taken from a heart warmed through about 5° C., and N_3 gives a tracing of the same, in which stimulation does not occur.

In the stronger tendency to cause omission of a ventricular beat, as well as in the frequent occurrence of an auricular contraction coincident with or succeeding the ventricular, when stimulation fails after ventricular maximum or in diastole, we see a marked contrast in the reaction of the venous sinus and the auricle to stimulation.

From the charts O_1, O_2, O_3 we see that the latency of the auricular beat varies. Thus stimulation occurring just at the end of auricular relaxation (O_1) causes an instantaneous reduplication, whilst during diastole proper it has a reduplication with a latency of ·2″. In the former case auricular induced systole precedes the ventricular, in the latter they occur at the same moment (O_2, O_3).

Contrast this result with the stimulation of the auricle itself in which reduplication occurs at once on stimulation, and ventricular

O_1.

O_2.

O_3.

Stimulation of Venous Sinus (maximal).

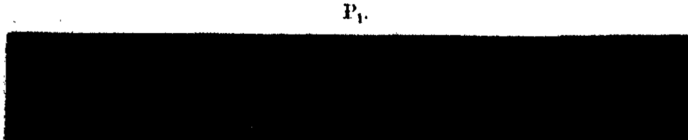
reduplication succeeds or occurs occasionally (stimulation at the end of auricular relaxation) in ·5″, followed by ventricle.

Appendix C. Strychnia on Frog's Heart.

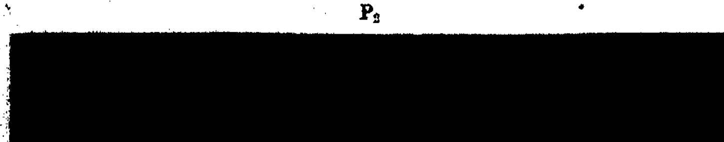
In order to test the correctness of the conclusion that strychnia lengthens the refractory period, we placed frogs in which the medulla and cord only existed on the cardiograph. The effects of stimulation were then observed, and subsequently a small dose of strychnia was injected into the dorsal lymph sac; as soon as the resulting spasm was well developed, stimulation was again applied, the strength of stimulation and the position of the electrodes remaining constant.

Thus, in fig. P_2, a frog's heart, in which active circulation was present, showed a refractory period through about one-half of the

P_1.



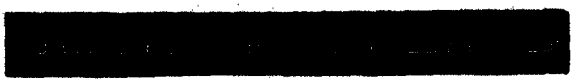
P_2



Stimulation of Ventricle.

1. Before injection of strychnia. 2. After injection of strychnia.

maximal maintenance of systole. In 3′, after the injection of a small dose of strychnia into the dorsal lymph sac, distinct spasm was present, and in 5′ fig. P_1 was taken, which showed that the refractory period had become prolonged, until relaxation of the ventricle had commenced.



Time-marker recording seconds. All tracings in Appendix C taken at this speed, except S and T.

It may happen that stronger stimulation before the maximum of systole is reached, causes an auricular beat, which precedes in normal rhythm the induced ventricular contraction. This is observed when the electrodes are placed near the base of the ventricle, or when stimulation is passed through the same portion of the heart from the float to an electrode placed beneath the heart upon the supporting shelf. After the maximum of systole, however, the auricular contraction succeeds the induced ventricular. Both these facts are demonstrated in fig. Q, in which this occasional increased auricular excitability is shown.

Auricular Stimulation.

Occasionally maximal stimulation applied to the auricle produces at all times an auricular contraction succeeded by a ventricular; more usually, however, this relationship exists only up to the maximum of systole (ventricular), and thereafter the induced auricular beat succeeds the ventricular.

Should stimulation cause an instantaneous auricular systole, then the ventricular reduplication has a latency of *nearly* equal value at all times at which it may occur, but should there be, as in fig. R_1, a considerable auricular latency (about 1″) then the ventricular latency is liable to great variations.

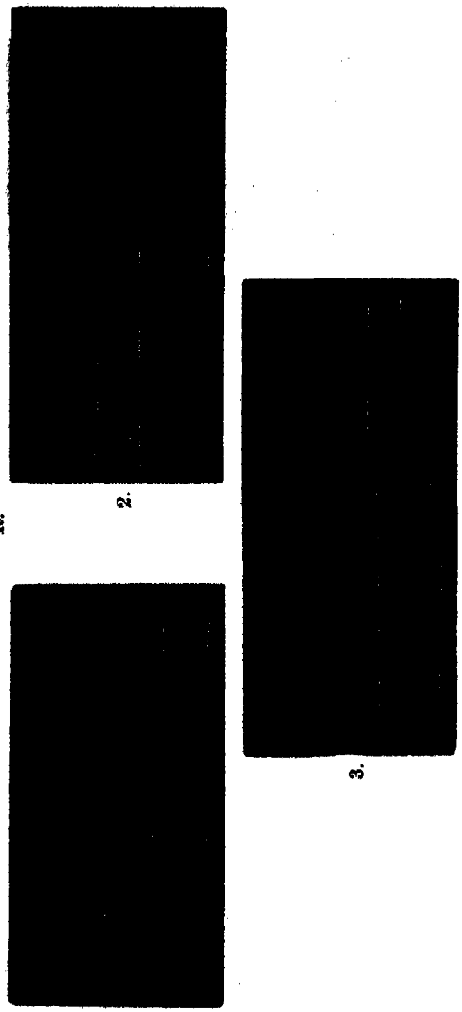
At the maximum of auricular systole, fig. R_2, we have an immediate auricular response, and a ventricular latency of ·4″; and in fig. R (3) there is an almost instantaneous ventricular systole, with an auricular latency of about 15″. The diastolic pause is the longer the later stimulation falls. In fig. R_1 it is ·9″; in fig. R_2 it is 1″·9; in fig. R_3 it is 2″·3.

Stimulation falling just after maximum of auricular systole, and at the commencement of ventricular systole, may cause in addition to the results enumerated, omission of the succeeding auricular and ventricular contractions, or reduplication of the auricular, but omission of the succeeding ventricular (fig. S).

Q.

Stimulation of Ventricle (maximal).

R.



1. 2. 3.

Stimulation of Auricle (maximal). 1, 2, 3, varying times of stimulation.

Thus the induced auricular contraction in this instance, instead of passing a motor impulse downwards to the ventricle, appears not only to check the reduplication, but greatly to prolong the diastole.

It is easily recognised from the auricular tracing that the induced contraction is one of the unfilled cavities (fig. S), but though little or no blood passes into the ventricle, a positive effect upon the latter is still produced.

S.

Stimulation of Auricle (maximal). Levers as in fig. Q.

Venous Sinus.

As regards the relationship of the auricular reduplication to the time of stimulation, we find the latency of the auricle occasionally varying in length, but usually it has very nearly equal values, except when the shock, falling during ventricular relaxation, calls forth a simultaneous auricular and ventricular contraction, and in this case latency is reduced. It is to be noted that this induced auricular contraction does not cause another induced ventricular systole: its further effects seem to be lost or dissipated.

At two points in Chart T, ventricular systole being advanced halfway and ·6 of the way to its maximum, the auricular latency is equal, and when at the end of ventricular relaxation the auricle contracts at the same time as the ventricle, the latency is still about the same. The time lost, therefore, in this case is in ventricular reduplication: either the impulse from the auricle is transmitted at different speeds at different times, or it meets at different times with variation in the excitability of the ventricle. The later in the systole the stimulation falls the less is the resistance to the transmission of the impulse or the greater the excitability of the ventricle.

The whole subject of the rhythmical contraction of the frog's heart and its stimulation and inhibition is a very complex and difficult one. The points upon which our present research seems to us to throw some light are the nature and mode of transmission of the stimuli which one cavity transmits to another in the ordinary process of rhythmical contraction. Marey's researches have shown that in the ventricle itself there is a time when stimulation applied to it has no apparent action; this time is, however, in many cases of very short duration and limited to the commencement of ventricular systole. At the commencement of ventricular systole stimulation without provoking contraction causes often a positive effect, namely, a greatly prolonged diastolic pause, which we have been inclined to regard as due to omission of a ventricular contraction.

F.

Stimulation of Venous Sinus (maximal).

It seemed of interest to ascertain whether a similar condition occurred in the other cavities of the frog's heart. We find that in the auricular stimulation about or shortly after the period of maximum contraction of the auricle may cause inhibition of the next auricular beat.

We have not yet succeeded in registering the contractions of the venous sinus with sufficient accuracy to enable us positively to determine the occurrence of a similar refractory period in the venous sinus itself, but the results we have obtained lead us to hope that we shall soon be able to do so.

Another interesting consideration is, whether the stimulus which each cavity of the heart transmits to the succeeding one, consists in the propagation of an actual muscular wave, or in the propagation of an impulse along the nerves. The observations of Gaskell have given very great importance to the muscular wave occurring in each cavity of the heart of cold-blooded animals as a stimulus to the contraction of the next succeeding cavity. Our observations appear to us to show that while this is an important factor, it is not the only one in the transmission of stimuli. We have observed that stimulation of the auricle rarely or never causes contraction of the ventricle unless the auricle also contracts. When stimulation of the auricle causes both itself and the ventricle to contract, the auricular contraction precedes the ventricular one in such a way that we might be justified in regarding the ventricular contraction as due to the propagation of the contractile wave from the auricle to the ventricle. It would also appear that a contractile wave may be propagated backwards, for on stimulation of the ventricle we have observed the contraction of the ventricle produced by stimulation has been succeeded by an auricular contraction such as might be supposed to be due to propagation of the contractile wave back from the ventricle to the auricle. While these observations appear to show that the propagation of the contractile wave from one cavity of the heart to another is of importance in keeping up the rhythmical sequence, we consider that stimuli are also propagated from one chamber of the heart to another through nervous channels:—thus we find that irritation of the venous sinus will sometimes produce simultaneous contractions of the auricle and ventricle, instead of the ventricular beat succeeding the auricular in the usual way. This we think is hardly consistent with the hypothesis that a stimulus consists of the propagation of a muscular wave only from the auricle to the ventricle.

As additional evidence we may notice the occurrence of an auricular beat followed by absence or inhibition of a ventricular beat as the result of stimulation of the auricle, or venous sinus. Moreover, we have noticed in the heated heart the occurrence of groups of regular beats in the ventricle in consequence of a single stimulation applied

to it, while the auricle has continued to beat with its ordinary unaltered rhythm undisturbed by the ventricular excitement.

It is not however our purpose to do more in this paper than state the results we have hitherto obtained, and we shall therefore reserve to a future communication the consideration of this and some other questions of importance closely allied to it.

Another question is the nature of the inhibitory influence exerted by one cavity of the heart upon another. Marey had shown that stimulation of the ventricle during a great part of the refractory period exercises an inhibitory instead of a motor action upon the ventricle itself. It might be supposed then that a stimulus of either kind, whether proceeding from the auricle in the form of a contractile wave, or a nervous impulse, might produce inhibition of the ventricle, provided the stimulus reached it during that part of the refractory period in which stimulation usually causes inhibition. From our observations it seems that the inhibition of the ventricle which may follow stimulation of the auricle is not due to the muscular wave propagated from the auricle and striking the ventricle during the refractory period. In fig. 6, we notice that the auricular contraction succeeded by ventricular inhibition occurs after the refractory period of the ventricle has passed; we must, therefore, look upon the inhibition as due to the propagation of a nervous impulse from one cavity to another. In the auricle we find that stimulation may produce inhibition of the auricular and ventricular beats, or of the ventricular beats alone. We may, therefore, suppose that the stimulus applied to the auricle acts upon two different nervous mechanisms; seeing that it is enabled to inhibit the ventricular beats without affecting the auricular ones, we are unable to say precisely what the effect of a single stimulus applied to the venous sinus is upon the sinus itself, but here we note that the same result will follow stimulation of the sinus, as of the auricle, viz., inhibition of the ventricular without inhibition of the auricular beat, or inhibition of both together.

As has been already pointed out by Professor Marey, the refractory period is increased when the heart is artificially cooled. We have also found that there is a prolongation of the time during which stimulation causes an inhibition or omission of the following systole.

It is very seldom that stimulation of the auricles or of the venous sinus causes a ventricular contraction without auricular systole preceding it in the ordinary rhythm. In this respect the action of the heart offers a contrast to the normal. Though the muscular wave started in the auricle is usually succeeded by a ventricular contraction, it may occasionally be succeeded by a ventricular inhibition, or auricular stimulation may be followed by inhibition of both auricle and ventricle.

The propagation of the wave in an upward direction, viz., from ventricle to auricle, is not so regular as in the normal heart, the time elapsing, when it does occur between the ventricular and auricular systole, bearing a relationship to the degree of cold produced. Whilst the ventricle is reduplicating in response to direct stimulation, the auricle may maintain its regular rhythm. Stimulation of the venous sinus almost invariably gives an auricular contraction at all times preceding the ventricular. It has been already shown that in the case of the normal heart stimulation in advanced diastole frequently causes a spontaneous auricular and ventricular contraction, or a ventricular beat preceding the auricular.

In the heated heart we have noticed, in addition to the excessive diminution or abolition of the refractory period in the ventricle already observed by Marey, that usually the refractory period in the auricle entirely disappears. A single stimulation of the ventricle sometimes gives rise to a series of contractions with incomplete relaxation intervening. After this has occurred, or after a simple reduplication has been caused, it often happens that the auricular beat occurring in normal sequence is not followed by ventricular, which seems to show a temporary state of exhaustion of the ventricle. In the heated heart the duration of a systole is so short that two beats immediately succeeding one another may be perfectly distinct, while, in the normal heart, the second one would have fallen within the time of the systole of the first, so that it could only have appeared, if it were possible at all, as an increase either of the height or length of the first systole. Inhibition occurs in the heated heart as well as in the normal, which is most frequently observed upon stimulation of the venous sinus, and it is frequently at this time associated with a reduplicated auricular contraction. The effect of strychnia is to prolong the refractory period of the ventricle. Stimulation of the ventricle is frequently succeeded by contraction of the auricle. There is an increased tendency for stimulation of the ventricle to induce a beat of the auricle preceding the ventricular systole. There is less tendency for the stimulation of the venous sinus or auricle to induce a beat of the ventricle succeeded by one of the auricle; and, indeed, this only occurs when the stimulus falls just at the end of the ventricular systole, *i.e.*, when the ventricle itself is most sensitive. These facts seem to indicate that the nervous channels are more active in transmitting stimuli, both downwards from the venous sinus to the auricle and ventricle, and from the ventricle back to the auricle.

In its effect upon the refractory period, and in the tendency it produces to maintain the regular rhythm, the action of strychnia agrees with that of cold, as shown in the present series of experiments; but, as we have already shown in a former paper,* its effect in causing

* "St. Bartholomew's Hosp. Reports," vol. xvi, p. 229.

the ventricle when arrested by a ligature applied around the junction of the venous sinus with the auricles to recommence pulsation resembles that of heat.

There are many other points on which we think that a fuller consideration of our experiments will throw light, but to take them up at present would involve too lengthy a discussion of doubtful points in the physiology of the frog's heart, and so we must reserve them for a future time.

OBITUARY NOTICES OF FELLOWS DECEASED.

In the decease of Professor WILLIAM STANLEY JEVONS, science and philosophy, both, have suffered a great loss. Since the departure of Boole and De Morgan—names which are ever on the tongue of philosophical mathematicians—no one has taken a more prominent part in the cultivation of symbolical logic than the accomplished man whose untimely death we have now to deplore. To the general public Professor Jevons was best known, perhaps, by his researches on our coal supply, his works on political economy, and his papers on various social questions of the day. His text-books for beginners have had an extensive circulation, and have proved highly serviceable to the class for which they were intended. His essays on currency and finance, on capital and labour, and on questions affecting the social life of the people, are also well known. But his reputation as a thinker and writer may be permitted to rest on his investigations into the principles of science and his contributions to a calculus of deductive reasoning. Bringing to his studies and researches not only a well furnished mind, but also a rare faculty for experiment and a taste for mechanical contrivances, he was enabled to embody the results of his intellectual labours in forms at once original and attractive. The instrument which he invented for the mechanical performance of logical inference, an account of which is given in the Transactions of our Society, could never have been devised by a man who was only a pure mathematician, or a pure logician. It is the "fruit of the grafting of an experimental genius on a philosophical genius." This peculiarity gave to his writings a special interest and value, and secured for them a wide circle of readers.

William Stanley Jevons was born at Liverpool on the 1st of September, 1835. His father, Thomas Jevons, was an iron merchant in that city, and his mother who wrote some poems, and edited the "Sacred Offering," was the eldest daughter of William Roscoe, the author of the well-known biographies of Lorenzo de Medici and Leo X. His earlier education was received at the High School of the Mechanics' Institution, and at a private school in his native city. Afterwards he was sent to London, where for twelve months he attended the classes of University College School. At the age of sixteen he entered University College and commenced the usual course of study in arts and sciences, matriculating in 1852 in London University with honours in botany and chemistry. In 1853 he received, through Mr. Graham, of the Mint, the appointment of Assayer to the Australian

Royal Mint, at Sydney. He had just before won the gold medal in chemistry, at his College, and was working at the time, along with his cousin, Dr. Roscoe, in the Chemical Laboratory of Professor A. W. Williamson. On receiving the appointment he at once threw himself into the intricate processes of gold and silver assay, and by a diligent course of study in London, under Mr. Graham, and at Paris, under the authorities of the mint there, quickly qualified himself for the duties of an office which he filled with conspicuous ability and success for five years. His leisure at Sydney was devoted to scientific pursuits, more particularly to the study of the meteorology of the district, a subject which up to that time had been very little cultivated. The results of his observations, extending over the whole period of his residence in the colony, were embodied in a pamphlet entitled— "Some Data concerning the Climate of Australia and New Zealand." During this period he published also a paper, "On the Cirrous Forms of Cloud, with remarks on other Forms of Cloud" ("Phil. Mag.," 1857), and another "On the Geological Origin of Australia and Earthquakes in New South Wales" ("Sydney Mag.," 1858).

But his tastes and powers fitted him for higher pursuits than those which chiefly occupied his time at Sydney, and in 1858 he resolved to relinquish his post there, and to return to England that he might resume and complete his University course. It was a bold step for him to take, for it involved the surrender of a career full of promise as to material advantages: but Jevons throughout life was animated by a pure and simple-hearted love for scientific labour. Writing to his cousin in January, 1859, he says: "I feel an utter distaste for money making, but on the contrary ever become more devoted to my favourite subjects of study. Perhaps you think I am too varied and desultory in my employments, which is partly true, but you know I am yet in a transition state. I told you long since that I intended exchanging the physical for the moral and logical sciences, in which my forte will really be found to lie. I like and respect most of the physical sciences well enough, but they never really had my affections. I should be glad indeed to follow out my subject of the *Clouds* and the movements of the atmosphere, because I feel sure I could place it in a new position altogether; perhaps I may spare time for this in England, but I shall make it a secondary thing. I have almost determined to spend a year at College before looking out for any employment in England; it might be worth while to take my B.A. (If I had had this degree before coming to this colony, I should vastly have improved my position in as well as outside the Mint.) I wish especially to become a good mathematician, without which nothing, I am convinced, can be thoroughly done. Most of my theories proceed upon a kind of mathematical basis, but I exceedingly regret being unable to follow them out beyond general arguments. I daresay it is

the general opinion of my friends in England that I am inexcusably imprudent in resigning £630 per annum. . . . But I ask, is everything to be swamped with gold? Because I have a surety of an easy well-paid post here, am I to sacrifice everything that I really desire, and that will I think prove a really useful way of spending life?"

Returning to England, he went on with his studies at University College, won various distinctions in his classes, and in 1860 proceeded to the degree of B.A. at London University. Two years later he graduated as Master of Arts with the Gold Medal in the branch of Logic, Philosophy, and Political Economy. Shortly after taking his B.A. degree he began to write for Watts's Chemical Dictionary. His articles, eight in number, relate to Clouds, Gold Assay, and Instruments employed in Chemical Analysis; they occupy altogether nearly sixty closely printed pages of the work. To the National Review (1861) he contributed an article on "Light and Sunlight"; and to the London Quarterly Review (1862) an article on the "Spectrum." Meanwhile his thoughts seem to have been turning from the physical to the mental sciences, and to questions in economics.

At the Cambridge Meeting of the British Association in 1862, Mr. Jevons communicated to the Statistical Section two papers, abstracts of which were printed in the Report, one entitled "On the Study of Periodical Commercial Fluctuations," and the other, "Notice of a General Mathematical Theory of Political Economy." About the same time he prepared two charts or diagrams published by Stanford, showing—(1.) The weekly accounts of the Bank of England, the quantity of notes in circulation, and the minimum rate of discount since 1844; and (2.) The price of the funds, the price of wheat, and the rate of discount since 1731. The diagrams represent to the eye and to the mind all the useful results of tables containing no fewer than 125,000 figures, which Mr. Jevons had compiled with great care and labour. When engaged on this work he was much struck with the enormous rise of prices about the year 1853, and was in consequence led to suspect a serious depreciation of the standard of value. Grappling with the difficulties of the inquiry, he examined with care the various causes of fluctuation in prices, seeking to distinguish between the temporary and the permanent. His views on the subject were embodied in an essay entitled "A serious Fall in the Value of Gold ascertained, and its social Effects set forth." In these papers will be found the germs of ideas and methods more fully developed in some of his later writings.

In 1863 Mr. Jevons received an appointment in connexion with Owens College, Manchester. That institution was then in its infancy, but full of vigorous life, and growing rapidly in strength and stature. Increasing demands were made upon the time of the

Professors, who had to perform, in addition to their own proper duties, those which are now assigned to lecturers and assistants. It became necessary, therefore, to extend the teaching powers of the College, and it was resolved as a first step to appoint a college tutor to assist the students in their various studies. This office Mr. Jevons was prevailed upon to accept. Few men could have been found so competent to fill it, his fulness of knowledge and versatility of mind qualifying him for the work in a very remarkable degree. "The multiplicity of the London University system," writes one of his colleagues, "had at no time any terrors for him, and I have known very few men so admirably endowed as he was with the continuation of force and elasticity necessary for confronting it." For three years he served the College in this capacity with distinguished ability and success. In 1866 some changes occurred in the *personnel* of the teaching staff, and Mr. Jevons was elected to a professorship. The chairs of Logic and Political Economy being united, were intrusted to him, and in this new position he found employment entirely adapted to his gifts and tastes. Logic had become his favourite but not exclusive study. Meteorology and the physical sciences had lost much of their hold upon him; but the theory of economics and problems connected therewith still continued to engage part of his attention. The influences that mainly contributed to mould the form and direct the progress of his logical investigations may here be noticed.

While residing in Australia he had read with care Mr. Mill's great work on Logic, and the interest then awakened in his mind was revived and strengthened on his return to England by listening to the lectures and reading the works of Professor De Morgan, that prince of teachers, to whom he often and warmly acknowledged his great indebtedness, as having inspired him with a deep love for logical method, and taught him to acquire those habits of exact thought and reasoning which are a better mental possession than any amount of mere knowledge. From De Morgan, also, he probably derived his tendency to look at logic on its mathematical side. But the man whose writings more, perhaps, than any other influenced the course of his logical speculations was Professor Boole. With the "Investigations of the Laws of Thought" Jevons first became acquainted in 1860, and from that date, throughout the remainder of his life, the science of logic occupied a prominent place in his studies. The boldness, originality, and beauty of Boole's system captivated him. As a generalisation of reasoning, he regarded it as vastly superior to anything previously known; but there were some portions of it that seemed to him dark and mysterious, and these he sought to separate from what he considered clear and unassailable. The calculus of 0 and 1, which plays so important a part in Boole's method, Jevons rejected on the ground that it represents other operations than those of common thought.

He attached to the sign $+$, as a logical sign, a somewhat different meaning from that which it bears in the works of Boole. He dispensed altogether with the indefinite class symbol v or $\frac{0}{0}$, and he imposed such restrictions as served to make the symbolical operations always interpretable in ordinary language. Thus, in place of the logical equation $x=vy$, he employed its equivalent $x=xy$, and so on. By means of these and other minor modifications he succeeded in producing a system by which logical problems may be worked out according to the general laws developed by Boole, but in such a way as to make all intermediate as well as final results interpretable. His earliest work on the subject is entitled "Pure Logic, or the Logic of Quality apart from Quantity: with Remarks on Boole's System, and on the Relation of Logic and Mathematics" (1864). This was followed by a paper in the "Proceedings of the Literary and Philosophical Society of Manchester" (vol. v, pp. 161–5, Session 1865–66), giving a brief account of his logical Abacus—a contrivance for reducing the processes of logical inference to a mechanical form. "The purpose of this contrivance," he says, "is to show the simple truth, and the perfect generality of a new system of pure qualitative logic closely analogous to, and suggested by, the mathematical system of logic of the late Professor Boole, but strongly distinguished from the latter by the rejection of all considerations of quantity. This logical abacus leads naturally to the construction of a simple machine which shall be capable of giving with absolute certainty all possible logical conclusions from any sets of propositions or premises *read off* upon the keys of the instrument. The possibility of such a contrivance is practically ascertained; when completed, it will furnish a more signal proof of the truth of the system of logic embodied in it. Still, the more rudimentary contrivance called the *Abacus* will remain the most convenient for explaining the nature and working of formal inference, and may be usefully employed in the lecture-room for exhibiting the complete analysis of arguments and logical conditions and the exposure of fallacies."

In a little book published in 1869, entitled "The Substitution of Similars," Professor Jevons simplified and extended his theory of reasoning. When logical propositions are expressed in the form of equations, the old distinction of subject and predicate is abolished, and the *dictum de omni et nullo* of Aristotle ceases to be applicable. Jevons therefore proposed to modify the ancient *dictum* and to replace it by the following:—*Whatever is known of a term may be stated of its equal or equivalent.* Or, in other words, *whatever is true of a thing is true of its like.* He held that all reasoning can be reduced to this fundamental principle. But the novelty in his views was most strikingly exhibited in his logical analytical engine, the construction of which was completed about this time.

Boole has shown that "the ultimate laws of logic, those alone upon which it is possible to construct a science of logic, are mathematical in their form and expression, although not belonging to the mathematics of quantity." Jevons advanced a step further, and showed that the processes of logic, like those of arithmetic or algebra, are purely mechanical, and can be not only exemplified but performed by a logical engine. A full description of this curious contrivance is given in a paper read before the Royal Society, in January, 1870, and printed in the "Philosophical Transactions," vol. 160, pp. 497–518. Other papers and treatises on Logic proceeded from his pen: "On a General System of Numerically Definite Reasoning" (Manchester Memoirs, 1872). "Primer of Logic" (1876). "The Principles of Science" (first edition, 2 vols., 1874; second edition, 1 vol., 1877).

The last-named work is a comprehensive treatise on Formal Logic and Scientific Method: it contains the matured results of Professor Jevons' researches on the subject, and is distinguished by great wealth and freshness of illustration. Almost every department of science is made to contribute examples in support or elucidation of the author's views on the theory of reasoning and the nature and limits of scientific inquiry. Perhaps the most original part of the work is that which treats of the "inverse logical problem," Jevons held that deductive reasoning gives the true type of all reasoning, and that induction in an inverse process bearing to deduction much the same relation that arithmetical division bears to multiplication, or evolution to involution. The direct or deductive problem is, Given certain relations among terms or notions, to determine by the application of the fundamental laws of thought, all the possible combinations which are consistent with these relations. The indirect or inductive problem is, Given the combinations, to determine all the possible relations from which these can be logically inferred. In other words, induction is a reasoning back from conclusions to possible premises. Whatever may be thought of this as a theory of induction, there can be no doubt that the inverse problem suggested by it is highly important. The solution of that problem in all its generality appears to be impracticable on account of the number and variety of combinations involved; but Jevons succeeded in obtaining a complete solution for two and for three classes; and the late Professor Clifford made a valuable contribution to the subject by determining the number of types of compound statement involving four classes. Clifford found the knowledge of the possible groupings of subdivisions of classes which he obtained by his inquiry, of service in some of his researches on hyperelliptic functions; and Professor Cayley subsequently suggested that this line of investigation should be followed out, owing to the bearing of the theory of compound combinations upon the higher

geometry. Those combinations possess an interest for the mathematician apart altogether from their logical significance. ("Manchester Proceedings," 1877, vol. xvi, pp. 89, 113.)

In 1867 Professor Jevons married Harriet Ann, daughter of the late John Edward Taylor, the originator, proprietor, and editor of the "Manchester Guardian." Three children were the fruit of the union, a son born in 1875, and two daughters, one born in 1877, the other in 1880. His domestic happiness, and the composure of mind resulting from it, facilitated largely the execution of his intellectual work. He confessed to it himself with his usual manly simplicity, and, as one of his friends says, "it was as if his very powers as an observer had derived a fresh and lasting impulse from the new associations which had become part of his life."

The question of the extent and the resources of the British coalfields was brought under public notice by the debates in Parliament on the Commercial Treaty with France in 1859-60. Attention was called to the importance of effecting a reduction in the National Debt, while coal and iron, the main sources of British wealth, were abundant. Hence arose the inquiry to what extent we might rely on the future produce of our coalfields. Professor Hull and others made estimates of the total quantity of accessible coal in the United Kingdom, and Sir William Armstrong, in his address at the British Association in 1863, gave prominence to the subject, pointing out that the problem to be solved is not how long our coal will endure before there is absolute exhaustion, but how long those particular seams will last which yield coal of a quality and at a price to enable our country to maintain her supremacy in manufacturing industry. Jevons attacked this problem with all the advantage gained from long experience in the collection and management of statistical details. His results were embodied in a treatise, entitled "The Coal Question: an Inquiry concerning the Progress of the Nation, and the Probable Exhaustion of our Coal Mines." (First edition, 1865; second edition, 1866.) Written with clearness, tact and vigour, and presenting the matter in a new and interesting light, the work was largely read; Jevons' conclusions were keenly discussed by journalists and reviewers, and attracted the attention not only of manufacturers and men of business but of politicians and statesmen of the highest order. A Royal Commission was appointed to inquire into the whole question, and Mr. Gladstone used Professor Jevons' calculations in support of his suggestion that a certain portion of the national revenue should be set aside as a reserve fund in payment of the National Debt.

Problems in applied economics had for Jevons a peculiar attractiveness, because of their bearing on the material welfare of the community. His devotion to abstract studies did not destroy his interest

in the progress of society, or in questions touching the practical life of men. While busied with researches on abstract principles, he always kept a window open to the outer world: witness his work on Coals, his papers on Currency and Coinage, Variation of Prices, the frequent Autumnal Pressure on the Money Market, and other kindred subjects, and his articles and addresses on questions of the day, published in the "Contemporary," the "Fortnightly Review," and elsewhere. Several of these scattered papers have lately been collected and republished in a separate volume under the title of "Methods of Social Reform;" and a glance at some of the headings will suffice to show the width of Jevons' interest in whatever affects popular progress—Amusements of the People, Free Public Libraries, Museums, "Cram," Trade Societies, Industial Partnerships, Married Women in Factories, Cruelty to Animals, The United Kingdom Alliance, Experimental Legislation and the Drink Traffic, State Parcel Post, &c.

A pamphlet on the Match Tax, which he wrote in 1871, is memorable as a skilful and courageous defence of a most unpopular measure. In his view the country had reached a critical point where "one great and true policy had been nearly if not quite accomplished;" and he feared that "without any strong guiding principle like that of free trade" before it, the nation was in danger of drifting instead of carefully steering in its financial course. "If one-half of the doctrines and arguments which were brought against the Match Tax should be accepted as really true and cogent, the balance of our financial system would be in danger of complete derangement." He therefore considered it important to subject to calm and impartial investigation the various opinions uttered during the heated discussion on the proposed new impost, and his pamphlet presents an admirable specimen of the way in which the truths of economics should be applied to questions of taxation.

But in his "Theory of Political Economy" (1871, second edition enlarged 1879), Jevons dealt not with particular applications, but with the general principles of the science; he laboured at the foundations. Dissatisfied with many of the views of Ricardo and Mill, he sought to construct the science on a new basis. Observing that "as it deals with quantities it must be a mathematical science in matter if not in language," he endeavoured to express quantitatively such notices as utility, value, labour, capital, &c., and he maintained that the employment of mathematical forms is conducive to clearness and precision of expression. It is curious to remark, however, that he did not attempt to develope those forms as a working process, and when it was pointed out to him that a little manipulation of the symbols in accordance with the rules of the differential calculus would often have yielded results which he had laboriously argued out, he contented himself with replying that he did not write for mathe-

maticians, nor as a mathematician, but as an economist, wishing to convince other economists that their science can only be satisfactorily treated on an explicitly mathematical basis. One who is both a mathematician and an economist bears the following testimony, as discriminating as appreciative, to the value and importance of Jevons' work in this branch of knowledge.

"Mr. Jevons," writes Professor A. Marshall, "was an economist of the highest order. In his 'Theory of Political Economy' he explains the nature of economic quantities, and their relation to one another. Work of this kind involves no startling discovery, but its effect is much greater than appears at first sight. It makes us master of our thoughts, and founds new empires in science. A small part of his work, which was warped by his antipathy to Ricardo, will probably die away. A small part also will lose lustre when Cournot's applications of mathematics to economics are better known. For indeed Jevons was, as he frankly confessed, not a skilled mathematician. Truly mathematical as is the tone of his best work, he was not at his ease when using mathematical formulæ. But the great body of his work is unaffected by these blemishes; the lapse of time will but add to its lustre, and it will probably be found to have more truly constructive force than any, save that of Ricardo, that has been done during the last hundred years. His contributions to statistics were widely known. The pure honesty of his mind, combined with his special intellectual fitness for the work, have made them models for all time. But it is in his essays on the applications of economics to the theory of governmental action that his full greatness is best seen. There is no other work of the kind which is to be compared to them for originality, for suggestiveness, and for wisdom. Almost every one of them contains some great new practical truth which the world is beginning to recognise, though but few persons know their obligations to him."

"Money and the Mechanism of Exchange" made its appearance in 1875, forming part of the International Scientific Series. In this work Jevons expounded the nature and functions of money, the principles of circulation, the various forms of credit documents, and the elaborate mechanism by which money exchanges are facilitated, adding some important historical notes and a discussion of certain technical points connected with the subjects treated of in the main body of the volume.

Jevons' connexion with Owens College extended over a period of thirteen years, during which the Institution steadily advanced in reputation and renown. Much of its progress was due no doubt to the liberality of its friends, but more perhaps to the genius and labours of its professors, and among these Jevons held a conspicuous place. In its new buildings and with its new constitution, the College

gave a considerable share of its government to the distinguished men who constituted its teaching body, and largely increased the importance of their corporate deliberation. Almost from the first Jevons proved himself a valuable member of the Senate, and at a somewhat critical period in the history of the College, he was chosen to serve as a representative on the College Council. "There were many qualities in him," writes one of his colleagues, "which more than justified the confidence reposed in him, but there was none for which he was more conspicuous than a comprehensive large mindedness which enabled him to look on questions with a view to something more than the immediate future. . . . Towards one change now actually effected, and in the opinion of many of us, deserving to be called a progress, he at first maintained an attitude of extremely well armed neutrality. I liked to attribute his coldness towards our University project to his loyalty as a member of the University of London; but I confess to having spent a very bad half hour when I made a final private attempt to argue him into a change of front. At the same time it is an instance of the sagacity which has marked his treatment of so many public questions, that he from the first (or nearly so) declared that the adoption of a constitution, such as that now actually possessed by the Victoria University, would be the right way towards the desired end.

In 1868 Jevons was appointed an Examiner in Political Economy in the University of London; in 1870 he was President of the Economic Section at the Meeting of the British Association in Liverpool; in 1872 he was elected a Fellow of this Society; and in 1874 and 1875 he was an Examiner for the Moral Science Tripos in the University of Cambridge. In the latter year the Senatus Academicus of the University of Edinburgh conferred upon him the honorary degree of LL.D., and in the following year he was appointed Examiner of Logic and Moral Philosophy in the University of London.

In 1876 he resigned his connexion with Owens College, and in the same year, on accepting the Professorship of Political Economy in University College, London, he removed to Hampstead, hoping there to have more leisure and greater facilities for the prosecution of his researches. The duties of his new position were less onerous than those to which he had been accustomed in Manchester; but academic work had never been very congenial to him, and lecturing, even on his favourite subjects, had become of late years somewhat of an irksome task. In a letter to the writer about this time, he speaks of the duties of the class-room as a "millstone" upon his health and spirits. Sometimes he had enjoyed lecturing, especially on logic, but for years past he had "never entered the lecture-room without a feeling, probably, like that of going to the pillory." "Now that I have been able to get rid of the burden," he adds, "I shall probably

be much better. I shall never lecture, speechify, or do anything of that sort again if I can possibly help it." Apart from special reasons, too, he found that the pressure of literary work left him no spare energy whatever. Besides the logical exercises which he had just finished and given to the world in a goodly volume, entitled "Studies in Deductive Logic," he had a large treatise on practical economy in full progress, a bibliography of logic in hand, and the analysis of "Mill's Philosophy" on his mind. He was also preparing a student's edition of the "Wealth of Nations," a preface to the English translation of Cossa's "Guide to Political Economy," and a volume on "The State in Relation to Labour" for Macmillan's English Citizen Series, besides new editions of some of his earlier treatises and various minor articles for the Reviews. Much of this work he actually accomplished before the waves closed over his vanished life. But much also remained unaccomplished. The "Principles of Economics," intended as a companion volume to the "Principles of Science," he did not live to complete.

In the summer of last year (1882) he went down with his wife and family to spend five or six weeks at Bexhill, a small village on the Sussex coast. Here he wrote an article on "Reflected Rainbows," which appeared in the "Field Naturalist." This was his last printed production. He had been accustomed in former years when visiting Bexhill to bathe in the sea, and being a good swimmer and familiar with the coast, he seems not to have apprehended any danger. But this season his wife had dissuaded him from the exercise, for he was not in good bodily health. The action of his heart was weak, and the close and continued intension of his mind on absorbing studies had much reduced his physical strength. On the morning of Sunday the 13th of August, he was walking with his wife and children on the beach, not far from the cottage on the cliff where they were staying. A man of warm domestic affections, he loved to be with the little ones, watching their innocent ways and participating in their simple pleasures. At length he turned to leave them, saying nothing of his intention to bathe; perhaps he formed the intention on his way back to the cottage, or after he had reached it. Taking a towel out of the house, he descended the cliff on the other side, and entered the water. No one else was bathing at the time, or within sight of the place; and the exact circumstances of his death can only be conjectured. It is believed that the sudden shock of the cold water, overstraining a weak heart, caused syncope, and that from the first plunge he was quite powerless. Some boys passing along the beach saw the body a few yards out, floating on the sea, face downwards, and at once gave the alarm. Among the persons attracted to the spot was a labourer residing on the hill who, twenty minutes before, had seen Jevons going down to the beach. When the body

was brought to land life was extinct. Within an hour and a-half of his leaving her, Mrs. Jevons' heard of her husband's death. His clothes and the unused towel were found lying on the shore.

Jevons was a man as remarkable for modesty of character and generous appreciation of the labours of others as for unwearied industry, devotion to work of the highest and purest kind, and thorough independence and originality of thought. The bequest which he has left to the world is not represented solely by the results of his intellectual toil, widely as these are appreciated not only in England but also in America and on the Continent of Europe. A pure and lofty character is more precious than any achievements in the field of knowledge; and though its influences are not so easy to trace, it is often more powerful in the inspiration which it breathes than the literary or scientific productions of the man. "That Professor Jevons will be missed," writes the editor of the "Spectator," "as one of the profoundest thinkers of our time on the philosophy of science, no one who knows anything of his writings will doubt. Yet he had other qualities, not always found in men of science, which made his character as unique as his intellect. At once shy and genial, and full of the appreciation of the humour of human life, eager as he was in his solitary studies, he enjoyed nothing so much as to find himself thawing in the lively companionship of intimate friends. Something of a recluse in temperament, his generous and tender nature rebelled against the seclusion into which his studies and his not unfrequent dyspepsia drove him. His hearty laugh was something unique in itself, and made every one the happier who heard it. His humble estimate of himself and his doubts of his power of inspiring affection, or even strong friendship, were singularly remarkable, when contrasted with the great courage which he had of his opinions; nevertheless, his dependence on human ties for his happiness was as complete as the love he felt for his chosen friends was strong and faithful. Moreover, there was a deep religious feeling at the bottom of his nature, which made the materialistic tone of the day as alien to him as all true science, whether on material, or on intellectual, or on spiritual themes, was unaffectedly dear to him." R. H.

Friedrich Wöhler was born on July 31, 1800, in the village of Escherheim, near Frankfort-on-the-Main, in the house of the pastor of that place, a brother-in-law of his mother. He received his first instruction in reading, writing, and arithmetic from his father; he went afterwards to the ordinary school, and later on he took private lessons in Latin and French, as well as in music.

Wöhler early showed a taste for experimenting and collecting, in which he was helped and encouraged both by his father and by his father's friend, Hofrath Wichterich, who took great interest in

science. Wichterich had some chemical and physical apparatus, with which he later on allowed the boy to experiment. Young Wöhler also collected all the stones which struck him as in any way remarkable. In 1814 he went to the Frankfort Gymnasium, which he continued to attend until he went to the University. Amongst his teachers at this period were several men who afterwards became distinguished—F. Ch. Schlosser, Grotefend, C. Bitter, &c. He was regular in his attendance at school, and was moved up to higher classes at the usual rates; but, as he himself used afterwards honestly to admit, he did not distinguish himself either by any marked industry or progress in knowledge. This may have been partly due to the fact of his having continued to occupy himself zealously with chemical experiments and collecting minerals. He worked so little at mathematics, for which he had little taste and aptitude, that he was afterwards obliged to get private lessons in it. He kept up a continual interchange of mineral specimens with several of his schoolfellows, amongst whom were Hermann von Meyer and Menge, who afterwards became a dealer in minerals, and who is well known for his travels in Iceland and among the Oural mountains. Wöhler in after years took to Menge at Hanover many a bag of hyaliths of his own collecting.

During this period of Wöhler's life Dr. Buch, a highly cultivated and intelligent man who gave private lessons and worked at chemistry, physics, and mineralogy, exercised a considerable influence on his scientific development; for years Dr. Buch let young Wöhler associate with him, and he it was who first incited the young man to study nature seriously. A kitchen in Buch's house served as a laboratory, in which on specified days they made experiments together.

Soon after the discovery of selenium, Wöhler had noticed the occurrence of this, at that time, rare substance in a sample of Bohemian sulphuric acid, and in consequence he sent for some of the ore from which it had been prepared The occurrence of selenium in this substance was proved, but it was not until 1821 that the result was made known to the world by Wöhler and Gilbert's "Annalen." The interest of Buch and Wöhler was also excited by the newly discovered metal cadmium, and they succeeded in preparing a small quantity, which Wöhler afterwards, when on a pedestrian excursion to Cassel and Göttingen, took with him to show to Stromeyer, the discoverer of cadmium, and to get it verified by him.

Wöhler's reverence for Blumenbach, whose "Handbook of Natural History" he had studied assiduously, gave him courage to visit Blumenbach on this occasion, and to take an opportunity of seeing his cabinet of specimens.

He became now more and more familiar with chemical processes, and whereas in the beginning he had merely had Hagen's "Experimental Chemistry," the book from which his father had studied, to

consult, he now had access to Dr. Buch's rich library. His room was gradually converted into a laboratory, full of glasses, retorts, flasks, stones, &c., all in the utmost disorder. Experiments requiring heat, which he could not carry on in this room, he used to make in the kitchen, where he pressed all the basins into his service. He likewise built up a small voltaic pile with some large Russian copper coins and zinc plates, and became acquainted with their property of decomposing water and causing shocks. This pile had not power enough to reduce potassium; but his desire to see and possess this remarkable metal, which he only knew from description, was so great that he endeavoured to procure it chemically, according to the method followed by Curandau, an operation which was not easy, but which, to his great satisfaction, succeeded. By way of a stove he made use of a large old tile of graphite, which Bunsen, the Master of the Mint, had given him. Bunsen had also lent him a pair of bellows, which Wöhler's sister used to blow.

Wöhler had many other interests and occupations besides these scientific experiments; he took regular lessons in drawing, to which his father, who was himself a good draughtsman, attached much importance, and when making excursions in the neighbourhood, in the Taunus, on the Rhine, &c., he always had his sketch-book with him. He also painted in oils. One of his hobbies was collecting old coins, of which he had a considerable number, as also of Roman urns, lamps, &c., which at that time were still frequently found in the old Roman camps at Gaddersheim, Maintz, and Wiesbaden. He also began a closer study of the German poets, under the guidance of the young painter from whom he learnt drawing.

Wöhler was as yet too young to have a clear insight into the great political events of that time, but as a boy he saw the triumphant entrance of Napoleon into Frankfort, and later on the entrance of the allied armies, and of the Cossacks, &c.

His father paid special attention to his physical development, to the strengthening and invigorating of his rather weakly frame by regular physical exercises such as riding, gymnastics, fencing, swimming, &c., and by shooting, both in winter and summer.

At Easter, 1820, he being then nearly twenty years of age, young Wöhler was sent to the University as *prima.* It had been previously settled by a family conclave that he should study medicine, not only because he wished it himself, but also because for several reasons this profession seemed to offer him the best prospects. He passed his first year of studentship in Marburg, where his father had studied before him, and where there were old family friends residing who could look after and advise the inexperienced student. He attended Ullmann's lectures on mineralogy, those on botany by Windewitz, Gerling's lectures on physics and mathematics, Bunger's lectures on anatomy,

as well as his dissection class. On chemistry, his favourite subject, there were no lectures during the first term, and in the second term he did not attend them, because Professor Wurzer had wounded his youthful feelings of ambition. To the very great annoyance of the mistress of the house where he lived, he had again turned his room into a laboratory, and had begun to make experiments on sulphocyanides and other compounds. He discovered iodide of cyanogen, at least it was to him a discovery, as he was not aware that Davy had already found it. In the joy of his heart he told Professor Wurzer of it, but the latter, instead of studying this substance, as new to him as it was to young Wöhler, reproached the youth bitterly that he, a student, should be on the look-out for discoveries, instead of attending to his medical studies. Through Dr. Buch these little researches were afterwards published in Gilbert's "Annalen."

At the end of a year Wöhler went to the University of Heidelberg, full of enthusiasm, even in anticipation, for Leopold Gmelin, who from this time forth was his favourite teacher as well as kindest friend and counsellor. Wöhler wished above all to hear Gmelin's chemical lectures, but Gmelin thought this would be a waste of time, and thus it came about that Wöhler never attended any course of chemical lectures. He gained, however, all the more by personal intercourse with Gmelin and the opportunity of working in his laboratory. He devoted to chemistry nearly all the time which his medical studies left him, and even towards the end of these studies, when practical medical work took up nearly the whole of his time, it seemed quite a necessity to him to go daily to the laboratory at least once. It was here that he had begun his investigation of cyanic acid, of which the earliest account had been published in Gilbert's "Annalen." It was to him a great gain that at this period Gmelin and Tiedemann were working at their joint chemical and physiological researches. Wöhler also enjoyed the special favour of Tiedemann, and owed to this excellent man his lively interest in physiology. It may have been partly due to the influence of Tiedemann that Wöhler undertook to write an essay, in competition for a prize offered by the medical faculty, on the passage of substances into the urine, for which purpose he performed numerous experiments, partly on himself, but mostly on dogs. He was so fortunate as to gain the prize. Though he might have made use of this treatise for a dissertation, he preferred that it should be published in Tiedemann's "Zeitschrift für Physiologie," 1824.

However, Wöhler's primary object was still to become a medical man, and latterly his inclination to this profession was increased by the closer acquaintance with its practical side which he gained in the clinical visits. He was more especially attracted by the midwifery cases, for which Nägele had a gift of interesting his pupils. Assuming that they would prefer this branch of practice to all others, Nägele

selected Wöhler and his friend and fellow-student, G. Spiess, to be present at every birth in the institution he attended during their last year of medical study.

In September Wöhler and Spiess went through their *sanitäts* examination and received the degree of Doctor in Medicine, Surgery, and Midwifery. Wöhler was now to begin travelling in order to visit larger hospitals, when Gmelin suddenly gave an entirely new direction to his life by earnestly advising him to give up the practice of medicine and to devote himself entirely to chemistry. Without taking much time to reflect, and certain of his father's consent, Wöhler gladly agreed to Gmelin's proposal. By Gmelin's advice, and encouraged by the favourable manner in which Berzelius had noticed Wöhler's earliest researches in his "Jahresberichte," young Wöhler applied to Berzelius for permission to work in his laboratory, which application was received in the most flattering manner.

Wöhler decided to travel to Stockholm by way of Lübeck. At Lübeck he took a passage in a small sailing-vessel bound for Stockholm; but the departure of the vessel was deferred from week to week, and he was actually detained in Lübeck for six weeks. He did not, however, find this altogether a waste of time, for he got an introduction through Menge, the dealer in minerals, to the scientific apothecary Kind, and in Kind's laboratory he worked at the preparation of larger quantities of the metal potassium. At length Wöhler sailed for Stockholm. After a very stormy passage he reached the Swedish coast at Dalarö, and on arriving at Stockholm a few days later received the warmest welcome from Berzelius.

A University laboratory is now a very different thing from what it was in those days. Wöhler was the only student in the laboratory of Berzelius. The laboratory consisted of two ordinary rooms, in which the chemists worked at deal tables. In a kitchen close by where their meals were prepared, and where the servant had to clean their chemical apparatus, stood a little furnace and a heated sand-bath.

Here Wöhler worked out some experiments on minerals, selecting chiefly those containing elements unknown to him, such as compounds of lithium and tungsten. Later on he returned to his experiments on cyanic acid. In these Berzelius took a special interest, as they seemed likely to cast a new light on the constitution of chemical compounds.

Besides working at these experiments, Wöhler assisted Berzelius in his beautiful research on hydrofluoric acid, his discovery of silicon, of boron, and of zirconium.

Whilst engaged during the day on these researches Wöhler used to work during his evenings at the Swedish language, translating Berzelius' papers into German for "Poggendorff's Annalen." In after years he used regularly to translate Berzelius' "Jahresbericht," as well as his "Lehrbuch."

Wöhler finished up his stay in the north by a scientific journey through Scandinavia in company with Berzelius and the two Brongniarts. The most interesting part of this excursion was a visit paid to the copper and silver mines of Fahlun, rich in mineral and geological treasures, and remarkable for the deep chasms which were formed centuries ago by the falling in of immense vaults. During this journey Wöhler was fortunate enough to get acquainted with several distinguished men of science, among them Sir Humphry Davy and Hisinger, whose mineralogical geography he afterwards translated into German.

With much regret Wöhler at last parted with his friend and teacher Berzelius and returned to Frankfort. His idea had been to settle down in Heidelberg as Privat Docent, but he was at once appointed lecturer in the newly established Gewerbeschule (Mechanics' Institute) at Berlin, and there he remained from 1825 to 1832, when family affairs decided him to move to Cassel.

It was in this same year 1825 that Wöhler became acquainted with Liebig, who was about his own age, and henceforth they were in constant intercourse. Liebig had become Professor of Chemistry at Giessen at the age of twenty-one, in the year 1824, and he followed Wöhler's work with all the greater interest because his own lay very much in the same direction.

These two investigators had each discovered a substance which according to minute analysis were composed of precisely equal quantities of the same elements. At that time it seemed like a paradox to say that two such different bodies as cyanic acid discovered by Wöhler, and fulminic acid which Liebig had examined, could have the same composition, for identity of composition in conjunction with difference of property was at that time quite unintelligible. Another research of far greater importance to the theory of organic compounds was an investigation made in the year 1832 of oil of bitter almonds and of benzoic acid. From these analytical experiments resulted a series of new compounds which were connected together by the fact of their all containing one particular group of atoms to which was given the name of benzoyl radical. Thus by one stroke was the connexion between all the members of this series of compounds made clear. The impression made by this research on contemporary chemists is plain from a letter written by Berzelius to the two investigators, in which he proposes for benzoyl the fanciful name "proïn" (daybreak), or otherwise "dawn," because with the discovery of this radical a new day seemed to dawn for chemical science.

A new day had indeed dawned for chemistry, and after fifty years the noon of this day shows us a vast number of substances with the most beautiful properties, some with the most brilliant colours, others of great value in medicine and surgery, all of which have been dis-

covered by methods of investigation similar to those pursued by Wöhler and Liebig.

We must not, however, omit to mention other researches carried out during Wöhler's stay in Berlin and Cassel, and which are no less important than the work he did with Liebig. His researches extended into every department of chemistry. In 1826 he succeeded in finding a method for obtaining nickel and cobalt from their ores free from arsenic, and thereby laid the foundation of an extensive industry. His appointment as teacher of the new manufacturing school gave him opportunities of helping industries connected with chemistry, and he became more conversant with such industries through a visit he paid to France in 1833, and to England in 1835.

In inorganic chemistry we owe to him the discovery of three new elements, that of aluminium (in 1827) of beryllium and of yttrium, besides various other beautiful discoveries.

We have mentioned that in 1823 Wöhler had gained a prize for a research in physiological chemistry on excretions from animal organisms. We must also mention his interesting discoveries in chemical physics. While examining the crystallisation of arsenious acid and of oxide of antimony, he noticed the remarkable fact that each of these bodies appears in two distinct crystalline forms.

But in organic chemistry we have yet to mention a discovery made by Wöhler in the year 1828, which, were it his only discovery, would entitle him to a place of honour in the history of that branch of the science, namely, the artificial formation of urea. In the previous century, as chemists became more and more occupied with animal and vegetable compounds, they had begun to classify chemical substances under the three headings of mineral, vegetable, and animal, but on account of the similarity between the two latter categories Lavoisier placed them in one class, which he called organic in contradistinction to the mineral or inorganic bodies.

Rouelle had found when examining urine in 1773 a beautiful crystalline organic substance. This was more minutely examined in 1779 by Fourcroy and Vauquelin and named urea. In 1828 Wöhler obtained these same long, white, glistening, needle-like crystals without the aid of any animal organism by gently heating the ammonia salt of cyanic acid.

This artificial production of an organic body at once abolished the doctrine of a distinct boundary line between organic and inorganic chemistry, together with the doctrine of vital force.

Shortly after this discovery Wöhler was appointed Professor of Chemistry to the Medical Faculty in the University of Göttingen, and about the same time he became Inspector-General of Pharmacies in the Kingdom of Hanover.

Wöhler now began a long and useful career of teaching. The

celebrated Göttingen Laboratory was built under his directions, and here he continued to work assiduously at his science. Some hundreds of researches testify to the diligence with which he worked during these years. Up to the year 1862 "Poggendorff's Annalen" already contained 225 papers by Wöhler. Students flocked from all parts of the world to study under his guidance.

Wöhler's work "Der Gundriss der Chemie," of which new editions have appeared from time to time until quite recently, has been translated into English, French, Dutch, Swedish, and Danish. His book "Mineralanalyse in Beijnelen" also passed through several editions. From 1838 Wöhler was Liebig's colleague in editing the "Annalen der Chemie und Pharmacie," and he published in conjunction with Liebig and others the "Handwörterbuch der Chemie" (Dictionary of Chemistry).

It is hardly necessary to mention that Wöhler received numerous honours and distinctions. Scientific societies were glad to number him among their members. The German Chemical Society elected him President. He was Knight of the Order "Pour le Mérite." He was elected a foreign member of the Royal Society in 1854, and received the Copley Medal in 1872. On his eightieth birthday his friends, pupils, and colleagues presented to him a beautifully executed relief in marble of himself, together with a gold medal struck in his honour.

Of late years Wöhler had ceased to work at research, although he continued to teach and occasionally to publish short papers. A letter written in 1878 to Dr. Victor Meyer gives a good idea of the spirit of quiet contemplation in which the old man rested from his labours. A few words are necessary to explain the occasion of this letter. In the summer of 1878 Victor Meyer had been giving to his students a somewhat detailed account of the discovery of urea, when it suddenly occurred to him that it was just fifty years since this synthesis had been accomplished by Wöhler, which fact he mentioned to his class. The consequence was that after the lecture the students sent Wöhler a telegram of congratulation, to which was appended a large number of signatures. The following letter came shortly afterwards in reply—

Göttingen, 26. Juni 1878.

Hochverehrter Herr College!

Sie haben mir die Ehre erwiesen in Ihrer Vorlesung meiner zu gedenken, und haben das Interesse an dem Gegenstande Ihres Vortrages bei Ihren Zuhörern so lebendig zu erregen verstanden, dass dieselben veranlasst wurden durch ein in liebenswürdigster Form abgefasstes Telegramm mir ihre Glückwünsche zu dem funfzigjährlichen Jubiläum der künstlichen Bildung des Harnstoffs darzubringen. Ichbitte Sie, Ihren Herren Zuhörern für diese überaus freundliche Aufmerksamkeit

meinen wärmsten Dank ausdrücken zu wollen und ihnen zu sagen, dass sie mich doppelt erfreut hat als ein Zeichen, dass ein alter Chemiker, dessen Kräfte nicht mehr gestatten, sich an dem weiteren Aufbau der Wissenschaft selbst noch zu betheiligen, von der jüngeren Generation nicht ganz vergessen ist, deren raschen Fortschritten und wundervollen Erfolgen aber immer noch seine Freude hat, gleich dem alten Fuhrmann, der selbst nicht mehr fahren kann, aber das lustige Peitschenknallen der jüngeren noch gerne hören mag.

Mit vorzüglicher Hochachtung,

Ihr ganz ergebenster,

Wöhler.

Wöhler passed the eighty-second anniversary of his birth last summer in good health, surrounded by his children, grandchildren, and great grandchildren, and in the course of the day received visits from various friends, colleagues, and pupils. He kept well through the month of August, and enjoyed sitting out in his garden during the warm summer weather. On the 18th September he did not go out on account of the coldness of the weather, and in the course of the evening was seized with a shivering fit. Fever soon came on, and on the 23rd September he breathed his last.

In accordance with his express wish his funeral was simple. The service was conducted in the house by the University preacher. A long procession of mourners followed the body to the cemetery.

Wöhler was twice married. His first wife, Franziska, daughter of Staatsrath Wöhler, whom he married in 1830, died in 1832, leaving him a son and a daughter. In 1834 he married Julie Pfeiffer, daughter of a banker in Cassel, by whom he had four daughters, and who survives him. Two daughters remain unmarried—one of whom, Fräulein Emilie Wöhler, long acted as her father's secretary.

A charming account of Wöhler's stay with Berzelius in Sweden, written by himself, is to be found in No. 12 of the "Berichte der Deutschen Chemischen Gesellschaft," 1875, under the title "Jugend-erinnerungen eines Chemikers."

Francis Maitland Balfour, the sixth child and third son of James Maitland Balfour, of Whittinghame, Haddingtonshire, and Lady Blanche, daughter of the second Marquis of Salisbury, was born in Edinburgh on the 10th November, 1851. His father died of phthisis in 1856, at the early age of 36, and his own health was for several years far from being strong, so that at times his friends were not without anxiety lest he too should be attacked with the same malady.

The beginnings of Balfour's scientific career may be traced to the influence of his mother, who endeavoured to cultivate in all her

children some taste for natural science or natural history. Under her guidance the children early formed a geological collection, beginning with fossils picked from the gravel in front of the house; in this collection, little Frank, as yet only seven or eight years old, interested himself greatly, and when offered the choice of a birthday present, begged for a large box, with trays and divisions, for holding fossils. The love for geology thus started grew strong in him during his boyhood; and, indeed, this continued to be his favourite study until he went to Cambridge. At the same time he was also drawn to natural history, making a collection not only of butterflies, as most boys do, but also of birds, having learnt how to prepare and preserve skins.

After spending some little time at Hoddesdon, in Hertfordshire, at the preparatory school of the Rev. C. B. Chittenden, he entered at Harrow in 1865. His love of science was daily growing stronger, but the ordinary school studies awakened very little interest in him; and at Harrow, as at Hoddesdon, he was not very successful in the routine class work. He was left-handed, and in his early days somewhat awkward in muscular exercises, though later on he overcame his deficiencies in this respect, and not only acquired great skill in anatomical and microscopical manipulation, but became an expert Alpine climber. A similar inaptitude to learn by mere imitation followed him into his school work; writing was a trouble to him, and, indeed, spelling no less so; hence his school career gave no promise of the achievements which were to come. Happily, even at that time, science was cared for at Harrow, and, in the scientific teaching of Mr. G. Griffith, Balfour found a satisfaction which he failed to get from his class work. Though these science studies were, so to speak, extra academical, he threw himself into them with great enthusiasm, and his future life was perhaps foreshadowed by the delight he showed when the Rev. A. E. Eaton, on a visit to Harrow, taught him the art of dissecting under water.

Geology still, however, remained his favourite study. In 1868, he sent up, in competition for a prize, an essay on the geology and natural history of East Lothian. This and another essay, by Mr. A. Evans, son of Mr. J. Evans, F.R.S., were considered to be of such unusual worth that Professor Huxley was specially requested to adjudicate on them; the two essays received equal prizes. The substance of his essay, Balfour, in conjunction with his brother, Mr. Gerald Balfour (afterwards with Francis a Fellow of Trinity College), subsequently elaborated into a paper "On some points in the Geology of the East Lothian Coast," which was published in the "Geological Magazine" for 1872.

In 1870 he left Harrow for Cambridge with the reputation of a boy not likely to distinguish himself greatly in ordinary University

studies, but still as one who by his great natural abilities, and especially by the force of his strong character, bade fair to make his mark somewhere.

In the October term of that year he went into residence at Trinity College, Cambridge, his college tutor being Mr. J. Prior. He early placed himself under the private tuition of Mr. Marlborough Pryor (who had just been elected the first Natural Science Fellow at Trinity College), and after passing, at Christmas, the "Previous Examination," devoted himself entirely to natural science. In Mr. Marlborough Pryor he found a friend of remarkably wide knowledge and sound judgment, who not only so well directed his pupil's studies, that at Easter, Balfour was, without hesitation, elected Natural Science Scholar at his college, but carefully matured those higher scientific qualities which are rarely tested in any examination.

After Christmas Balfour attended the lectures of Dr. Michael Foster, who had been called to Trinity College, Cambridge, as Prælector, at the same time that Balfour entered as a student. In a course of lectures on embryology, given by the latter in the following Easter term, Balfour was especially interested, and he soon after definitely made up his mind to devote himself to the study of animal morphology.

Before long he was invited by Dr. Foster to join him in preparing for publication the lectures on embryology, which the latter had given, and with that end in view began original inquiry by attempting to investigate certain obscure points in the history of the chick. The results of these studies were subsequently published in the "Quarterly Journal of Microscopical Science" (July, 1873), as three papers "On the Development and Growth of the Layers of the Blastoderm," "On the Disappearance of the Primitive Groove in the Embryo Chick," and "On the Development of the Blood-vessels of the Chick." Some of the views enunciated in these early papers were naturally modified by later experience, but the observations on the primitive streak are interesting as forming the starting point of views which Balfour developed more fully afterwards.

These investigations, and others not recorded in any special papers, but made use of in the little work "Elements of Embryology" (1874), by Dr. Foster and himself, occupied so much of Balfour's time and energy, that he was unable to do much in the way of formal preparation for his degree. Nevertheless, he nearly succeeded in obtaining the first place in the Natural Sciences Tripos in December, 1873; this fell to Mr. H. Newell-Martin, now Professor at Baltimore, United States, America, Balfour being placed second. Immediately after his degree he started, in company with his friend Mr. A. Dew-Smith, for Naples, to occupy at the Stazione Zoologica of Dr. Anton Dohrn one of the tables at the disposal of

the University of Cambridge, and began those researches on the development of Elasmobranchs with which his name will be ever associated. Before leaving Naples in the following summer he had not only obtained some striking results as to the development of the layers of the blastoderm, but had in reality solved the difficult problem of the nature and origin of the urogenital system of vertebrates. The work done in Naples showed talents of so high an order that in the following October (1874) Balfour was on account of it unhesitatingly elected a Fellow of his College. The following winter he spent in a visit to South America with his friend Marlborough Pryor, but returning in the spring of 1875 resumed his investigations at Naples.

He had, at the meeting of the British Association at Belfast, in August, 1874, made a brief statement of his researches, and in October of the same year published a preliminary account in the "Quarterly Journal of Microscopical Science." In the course of 1875 he contributed to the "Journal of Anatomy and Physiology" a paper on the Urogenital Organs of Elasmobranchs, and laid before the Royal Society an account of the development of the spinal nerves in those animals, which was subsequently published in the "Philosophical Transactions." He now commenced in the "Journal of Anatomy and Physiology" a series of papers (the first of which was published in January, 1876), giving a complete account of the development of the Elasmobranchs. These were afterwards (1878) republished as a monograph.

In the course of the summer of 1875, arrangements were made for him to deliver at Cambridge a course of lectures on animal morphology. These he began in the following October, and soon after his position was secured by his being appointed as lecturer to his College, though his lectures as before continued to be delivered in the University buildings, and to be open to all students of the University. Beginning with a very small audience, he rapidly drew students to him by the powerful way in which he taught as well as by the interest which he showed in the progress of each individual pupil.

In spite of the time and energy taken up in organising and carrying on these lectures, he himself always assisting in the accompanying practical exercises, he pursued with unflagging ardour his original investigations. No sooner had he finished the monograph on the Elasmobranchs, than he set himself to write a complete treatise on Comparative Embryology. The value of this work, the first volume of which, treating of invertebrates, appeared in 1880, and the second, of vertebrates, in 1881, cannot easily be exaggerated. It is not only a masterly digest, in which the enormous number of observations made during the last quarter of a century, and especially during the last decennium, are marshalled in proper order, and their nature and

significance clearly and acutely explained; it also contains, one might say in almost every page of the two thick volumes, the record of original, often laborious inquiries, to which the author was stimulated sometimes for the sake of verifying the statements of other observers, but more frequently for the purpose of solving morphological problems which presented themselves to him as the work went on. Some of the larger results, which thus sprang out of the work, elaborated as inquiries carried out by himself, or through him by his pupils, were published as separate papers; but even when these are accounted for, there still remains imbedded so to speak in the work, an enormous amount of original work, in the form both of new facts observed by himself and of luminous interpretations of the facts which others had recorded, but whose true meaning others had failed to see.

Parts of his vacations were spent in trips or longer voyages, for the sake of health and amusement. In this way he twice visited Finland, and spent a Christmas in Greece; but he always contrived, even in the midst of his pleasures, to make his holidays help in his work. He paid repeated visits to Naples, and indeed he was from the beginning one of the staunchest and most valuable friends of Dr. Dohrn and of the Statione Zoologica. He was on a visit to his friend Prof. Kleinenberg, at Messina, in the Christmas of 1881, buoyant with the feeling of relief at having completed the Comparative Embryology, when, staying at Naples on his way, he found a pupil who had been sent by the University of Cambridge to study at the Statione Zoologica, lying sick of typhoid fever at Capri. With characteristic unselfish tenderness, Balfour set himself to nurse his pupil until the young man's friends could arrive. There can be no doubt that he thus caught the malady, for in January (1882), soon after his return to Cambridge, he himself was laid up with an attack of typhoid fever, which threatened to be severe, but happily passed off well.

Some time before this great endeavours had been made to induce him to become a candidate for the chair at Oxford left vacant by the lamented death of Professor G. Rolleston, and afterwards he was even more vigorously urged to accept a nomination to succeed the late Sir Wyville Thomson in the chair of Natural History at Edinburgh, perhaps the best endowed and most conspicuous biological chair in the United Kingdom. He refused, however, to leave his own University, though his position there was simply that of a college lecturer and he had no post in the University itself. Moved by the peril of thus losing one of the brightest and most promising of its *alumni*, the University, at the instigation of Balfour's friends, took a most unusual step, and the Council, with the approval of the whole University, instituted a new chair for Balfour himself, and in March, 1882, he was elected Professor of Animal Morphology.

On his return from a visit to Naples in the summer of 1876, he

stayed for some little time in Switzerland, and apparently then was first taken with the love of Alpine climbing, though it was not till the summer of 1880 that he made any difficult ascents. The fondness for this exercise, the beneficial effects of which on his health were most striking and encouraging, grew upon him in the following years. In the summer of 1881 he passed some weeks in Switzerland, in company with his brother, Mr. Gerald Balfour, and by his feats on that occasion placed himself at once in the front rank of Alpine climbers. In the summer of 1882 he thought, and even the most cautious of his friends thought with him, that the Alpine air and mode of life would remove the last traces of weakness which the typhoid fever had left behind, and in June he started, full of spirits, for Switzerland. After some two or three weeks or so of climbing, during which he felt his strength quite come back, the old remedy acting with its usual charm, he set off from Courmayeur, with his guide, Johann Pétrus, on Tuesday, July 18, to ascend the neighbouring Aiguille Blanche, a hitherto virgin peak. But he never came back alive. On the following Sunday an exploring party found his remains and those of his guide lying high up on the mountains at the foot of a couloir. The exact time and manner of his death will never be known, but it is probable that the fatal fall took place on Wednesday, the 19th; it is almost certain that death was instantaneous, and it is the opinion of some that in the accident the guide fell first and carried Balfour with him. The body was brought home to England and buried at Whittinghame.

To describe in a few words Balfour's contributions to biological science is a difficult or rather an impossible task, for brief as was his life, his active brain had traversed a large and varied area of thought and observation. The leading idea which guided him in all his work was to use the facts of embryology to explain the development of animal life, to make the evolution of the individual throw light on the evolution of races and kinds. This idea was of course no new one; indeed it has guided nearly all morphological inquiries since Darwin's labours, as, in a way, it did some before. But it was Balfour's distinguishing feature and merit that while his lively imagination opened up to him all manner of bold views and striking hypotheses, a strict logical sense forbade him to confound a mere possible or likely suggestion with a proven truth. The value of his work lay in this, that when he had conceived an idea he left no means untried to test its worth, and the charm of his writings consists as well in the clear and strong way in which he lays down proofs of the things which he considered proven, as in the frank candour with which he sets forth the difficulties of a probable, but as yet uncertain, opinion.

Perhaps the most striking and complete of his works is that in which in a masterly way he furnished adequate proof of the view (which had occurred to others as well as himself) that the urogenital

organs of vertebrates are the derivatives of the segmental organs of Vermes, thus forging a strong link in the chain which holds together all animal life. Of still wider import, and perhaps still greater value, was his elaboration of views, begun in his apprentice work on the Primitive Streak, and continued even into those last investigations which his sad death left unfinished, on what may be spoken of as the general formation of the complex animal bodies, and especially on the relation of the alimentary canal to other parts. Balfour early saw that certain vanishing grooves and holes and burrows in the embryos of higher vertebrates were the traces of the ways in which these higher forms had been evolved from lower ones, and all his work through he left no stone unturned, *i.e.*, no specimen or section unsearched, in his efforts to fill up gaps of evidence, and thus to make the whole story clear.

The same guiding principle, the same logical method, the same clear distinction between the proven and the probable are also seen in his remarkable memoir on the Spinal Nerves of Elasmobranchs (which threw almost as much light on the genesis of the vertebrate nervous system as his earlier work did on the urogenital organs), and, indeed, are conspicuous in every one of his separate papers, including the unfinished fragments on Peripatus, as well as in every page of the incomparable "Comparative Embryology." He was unwearied in his labours, sitting for hours together preparing and examining section after section; but others have been as unwearied as he. To him belonged that part of genius, which kept him from being buried beneath accumulated facts, and gave him the power to seize at once upon the new salient fact as soon as it appeared, to develope its meaning, and to carry its teaching home to others by solid irrefragable reasoning.

Balfour will be known hereafter as a brilliant morphologist, as one who busied himself with high questions of theoretic import; but there was also another side to even his biologic character. Much of the progress of biology has been due to the labours of men to whom perhaps more rightly belongs the title of naturalist; men who often do not vex themselves with the more abstract problems of morphology, but born with an innate love of living things, quietly gather facts and work out truths, putting together their results in the guise for the most part of some taxonomic inquiry. Naturalists of this stamp are generally born such, not made, whereas a man of adequate mental strength may become an accomplished morphologist without feeling any real sympathy with concrete animal life; he may be carried on by the mere interest of purely intellectual questions. Now Balfour, like his master, Darwin, was a born naturalist; his knowledge and appreciation of the concrete characters of the individual were as striking as the power which he displayed in dealing with abstract

theories. He "knew his British birds" as few others did; nor was his knowledge of species limited to these; and, indeed, had he lived to complete the monograph on Thysanoura, for which he had been long collecting materials, this would probably have placed him in the very foremost rank of British taxonomists.

He was, as we have said, a geologist who became a biologist, but his sympathies with all science remained wide and deep. His knowledge of physics, as far as it went, was singularly clear and sound; his judgment on intricate physiological questions was often of the greatest use to his friends at Cambridge. He was an ardent politician, he possessed great sympathy with art, and in the business of his College and University, as well as in other matters, showed administrative abilities of the very highest order.

He had in him all the making of a great man, and his greatness would have been felt whatever had been the things to which he turned his hand; and no small part of the power with which, even in his few years, he had already begun to influence others, sprang from those qualities which, by a common but misleading analysis, we call moral as distinguished from intellectnal. According to the degree to which their intimacy with him grew, those who got to know him were charmed with his kindly courteousness, fascinated with his brilliant, cheerful, often playful, companionship, held fast by his warm-hearted, steadfast friendship. The feeble found him a patient helper; meanness, untruthfulness, and conceited stupidity were alone able to provoke him to anger, and to show what powers of scorn and sarcasm lay hidden in him.

Though he died so young, his great merits were already rapidly gaining due recognition. He became a Fellow of the Society in June, 1878; on him, in November, 1881, was bestowed one of the Royal medals, and at the same time he was elected member of Council. In the spring of 1881 he received the honorary LL.D. of Glasgow, and at the time of his death he was President of the Cambridge Philosophical Society, having been elected to that position in October, 1881.

We have already spoken of the eagerness shown both at Oxford and Edinburgh to gain him, and of the steps taken at his own University to keep him. For one brief term only was he professor, and even during that term, owing to his previous illness, he gave no lectures; now, instead of his bright presence, there is left at Cambridge only his memory and the wish to carry on the work he left undone.

M. F.

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END OF THE THIRTY-FIFTH VOLUME.

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.